

HARVARD STUDIES
IN
CLASSICAL PHILOLOGY

*EDITED BY A COMMITTEE OF THE CLASSICAL
INSTRUCTORS OF HARVARD UNIVERSITY*

VOLUME XXXIV



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PREFATORY NOTE

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EDWARD KENNARD RAND,
CHANDLER RATHFON POST,
WILLIAM CHASE GREENE,

} EDITORIAL
COMMITTEE.

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THE PELOPONNESOS IN THE BRONZE AGE ¹

BY J. PENROSE HARLAND

Fellow of the Institute at the School at Athens, 1920-21

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I. INTRODUCTION ²

1. *The Helladic Civilization*

THE recent excavations on the mainland of Hellas have been so rich in results that we are now enabled, not only to trace the artistic development of the peoples who inhabited the Peloponnesos in the Bronze Age, but also, by combining these new archaeological data with the traditions and linguistic evidence, to construct a history of the peoples themselves. Because we can now speak of the "history" of this early period, the term "prehistoric" becomes automatically inappropriate, and hence the more satisfactory appellation "Bronze Age" has been used in this paper to define the period between the Neolithic Age and the Iron Age, that is, approximately the period from *circa* 2500 B.C. to *circa* 1100 B.C.

¹ A paper prepared at the American School of Classical Studies in Athens, in accordance with the regulations governing Fellowships.

² This paper was written while I was a Fellow at the American School of Classical Studies at Athens (1920-21); but it has been in part revised and entirely rewritten in the summer of 1922. As for the archaeological evidence, I must acknowledge my indebtedness first and above all to Dr. Carl W. Blegen, Assistant Director of the School, and then to Mr. A. J. B. Wace, Director of the British School at Athens; from these two I acquired practically all that I know about early pottery, and I take this means of thanking them for all the information that they have given me at Zygouries and at Athens, and also for that contained in letters written since my

But a still more important result of these excavations was the discovery that this early civilization of the mainland was different from that of Krete, and that, therefore, the system of the Kretan-Minoan classification cannot be satisfactorily applied to the culture of the Helladic mainland. It was Helladic, not Minoan, and (for a while at least) Hellenic, not Kretan. For this reason, Messrs. Wace and Blegen devised the "Helladic" system of chronology and terminology,¹ and their selection of the term "Helladic" is a most happy one.

Doubtless in time this system will in the main be accepted by all scholars, and so it does not seem necessary to demonstrate the superiority of the term Helladic over the hitherto better known but less applicable term Minoan. Even a cursory study of the finds from the mainland excavations is enough to convince one that the Helladic culture is as distinct from the Minoan culture of Krete as the latter is from the culture of the Kyklades. To quote the authors of the Helladic system, "These three divisions, Early Helladic, Early Cycladic and Cretan Early Minoan Ware are all branches of one great parent stock which pursued parallel, but more or less independent courses . . . the Mycenaean civilization . . . is the fruit of the Cretan graft set on the wild stock of the mainland."² Not until the end of the Bronze Age is it possible to apply the term Minoan to the mainland culture, and even then the application would not be correct, for Mykenian is not synonymous with Minoan.

The criticism has been made that the Minoan periods are in part arbitrary or stylistic divisions, and that they are not always in accord with the stratification of the sites excavated, even in Krete. But the return from Athens. As this is a history, I could not omit mention of Professor William K. Prentice of Princeton University, who is largely responsible for my entering the field of Ancient History, and who has ever been most kind in giving me the benefit of his valuable criticism, at home and abroad. I also wish to express my thanks to Professor W. S. Ferguson for his criticisms, and especially to Professor George H. Chase of Harvard University, who has been so generous with his time in making corrections and suggestions during the preparation of this paper for the press. And lastly, I desire to thank the editors for their kindness, greatly appreciated, in permitting me, contrary to their practice, to retain the Hellenic proper names as I had written them in the MS.

¹ Wace and Blegen, *The Pre-Mycenaean Pottery of the Mainland*, in *British School Annual*, XXII (1916-1918), pp. 175-189, Plates VI-IX.

² *Ibid.*, pp. 180, 188.

Helladic periods correspond to the stratification noted in the excavations conducted by Dr. Blegen at Korakou near Korinth.¹ This site yielded an undisturbed stratification, and supplied a pottery sequence from the beginning to the end of the Bronze Age. The evidence from Korakou has in the main been corroborated by the results of the American School's excavations, in the spring of 1921, at Zygouries² in the valley of Kleonai, and by investigations carried on at other sites in the Korinthia and the Argolid, especially at Mykenai, where the British under the direction of Mr. Wace have been digging for the past two seasons.³

In another way the Minoan system fails to be applicable to the civilization of the Helladic mainland. The Minoan periods do not synchronize with the Helladic periods (and strata), as a glance at the comparative chronological table prepared by Wace and Blegen will show.⁴ The Middle Helladic Period does not begin until the First Middle Minoan Period has passed. And there is no mistaking the abrupt and sudden change from the Early Helladic to the Middle Helladic Period. It occurs, as computed by comparative dating with Egyptian finds of the XIIth Dynasty, at *circa* 2000 B.C., that is, at

¹ Carl W. Blegen, *KORAKOU, A Prehistoric Settlement near Corinth* (1921). This veritable textbook on Bronze Age pottery will hereafter be referred to as *Korakou*.

² Zygouries, a "prehistoric" site situated about two miles S. E. of the akropolis of Kleonai and one mile W. of the village of Hagios Basileios, was excavated in the spring of 1921 by the American School, under the direction of Dr. Blegen. Preliminary report in *American Journal of Archaeology*, 1921, p. 298; v. also *Art and Archaeology*, XIII (1922), pp. 209-216.

³ Report by Mr. Wace in article, *Archaeology in Greece, 1919-1921*, in *Journal of Hellenic Studies*, 1921, pp. 262-265.

⁴ *Korakou*, p. 121; Wace and Blegen, p. 186. Dr. Hazzidakis, in the light of his investigations at Tyllissos, Krete, has regrouped the sub-periods of the Minoan system of Sir Arthur Evans, so that this modified system of Minoan chronology is, in the main, in accord with the Helladic system, especially with the modified form of it that I have proposed in this paper. However, even this new Minoan system of classification is not appropriate for the culture of the Helladic mainland. The Minoan and the Helladic were two different civilizations. V. Joseph Hazzidakis, *Tyllissos, à l'Époque Minoenne, Étude de Préhistoire Crétoise* (1921); cf. L. Franchet, *Rapport sur une mission en Crète et en Égypte* (1912-13), *Céramique antique, Recherches techniques appliquées à la chronologie*, in *Nouvelles Archives des Missions Scientifiques*, XXII (1916). I had proposed the modification of the Helladic system before I had seen either of these two works.

about the beginning of the Second Middle Minoan Period. In other small points the Minoan system can be shown to be unsuitable to the mainland or Helladic civilization; but it is unnecessary to say more here. The Helladic system can be satisfactorily applied to the mainland civilization, the Minoan cannot.

The Helladic system, however, must be modified before it can become universally accepted as the standard or authoritative system of classification. Although the subdivisions are based upon stratigraphic evidence, and are relatively correct chronologically, yet the major periods (the last two at least) are artificial, for the Helladic system has as its basis the Minoan system of Sir Arthur Evans.¹ Hence, the 'break' which serves as the line of separation between the Middle Helladic and the Late Helladic Periods is set at *circa* 1600 B.C. (at the beginning of LH I, or LM I). But, as I shall try to demonstrate below, a really distinct 'break' occurs at *circa* 1400 B.C. (that is, between LH II and III), and it is more than a stylistic 'break'; it appears to be a 'break' in the cultural and ethnic sequence of the inhabitants of the Peloponnesos. I have, therefore, taken 1400 B.C. as the dividing point between the Middle Helladic and the Late Helladic Periods. Except for this change, I have followed the Helladic system of Wace and Blegen practically *in toto*.

2. The Evidence

The evidence for the history of the Peloponnesos in the Bronze Age may be classified in three groups: (1) Archaeological evidence, (2) Linguistic evidence, and (3) the Traditions. As is natural in the case of a history of this early period, the archaeological evidence is greatly in excess of the other kinds, and of this first group the pottery forms the chief part. Architecture and small finds afford important but scanty evidence.

It is unnecessary to discuss the importance of the study of pottery to early history. Pottery not only affords chronological data, but may show that trade-intercourse, direct or indirect, existed between two peoples or between two lands at a certain period, and may indicate an influence which one civilization had upon another people. It

¹ *Korakou*, p. 120.

does not usually afford a clue to the ethnic character or provenance of the people using the pottery. Change of pottery by itself does not ordinarily indicate a change of race. But when one type of pottery is suddenly superseded by an entirely new type, and there are concomitant indications of a change, — such as a layer of ashes between the two strata in question, — then the change in the pottery may indicate the incursion of a new people. To anticipate a few pages, such seems to have been the case in the Peloponnesos at the beginning of the Second Millennium B.C., that is, at the beginning of the First Middle Helladic Period when Gray Minyan ware suddenly appears.

3. *Nomenclature*

It is a problem in itself to devise suitable names for the peoples who in succession occupied the Peloponnesos in the so-called “prehistoric” period, the Bronze Age. We have no written records to refer to, and even if we had records of this early period, we should hardly expect to find all the people of a big movement grouped under one name. A common, all-embracing, ethnic name is the last goal attained, not the starting point in the evolution of ethnic names.¹ We must thus think of each invasion as a migrating horde, made up of several tribes. Hence the historian of this early period must either avoid the use of all ethnic or tribal names and have recourse to cumbersome paraphrases, or invent names. Such names are often fanciful; but better a fanciful name than no name at all. It is not necessary that the invented name have any historical basis. It should be thought of as merely a label for the convenience of the writer and the reader. The sole requirement should be that the name adopted should not be liable to confusion with some other name, and names with a special local significance should be avoided if possible.

It is to be hoped that, as time goes on and new evidence comes to light, the historians and archaeologists will agree upon a suitable uniform system of terminology or nomenclature. In this paper I shall merely suggest names, and, going on the principle that a name of a “prehistoric” people is merely a label of convenience, I shall offer no apology for them — now.

¹ Cf. the statement of Eduard Meyer about language and states, in his *Geschichte des Altertums*, I, 1 (1921), sec. 39, p. 79.

I shall here give the names or labels that are to be used for the various strata of peoples which appear in the early history of the Peloponnesos, but shall reserve for their proper places, respectively, the reasons that led to the selection of these names or labels.

EARLY HELLADIC PERIOD	"AIGAIANS"
MIDDLE HELLADIC PERIOD	"MINYANS"
LATE HELLADIC PERIOD	"ACHAIANS"

4. Chronology

The following table of chronology for the early history of the Peloponnesos and the Helladic civilization has been derived from the study of the pottery and the stratigraphic evidence, and from synchronisms with Egypt. In the number of the periods and in the dates this table is almost identical with that presented by Dr. Blegen in his *Korakou*.¹ But there is this important difference: I include his Late Helladic I and II in the Middle Helladic Period, and make the Late Helladic Period begin with his Late Helladic III. Of course, this chronology is for the most part relative, but the dates for the beginning of Middle Helladic I and III and of the Late Helladic Period are approximately correct, as they have been derived from the synchronisms with Egypt.²

HELLADIC CHRONOLOGY	
NEOLITHIC AGE.....	Down to ca. 2500 B.C.
EARLY HELLADIC PERIOD.....	ca. 2500-2000 B.C.
MIDDLE HELLADIC PERIOD.....	2000-1400 B.C.
Middle Helladic I.....	2000-1800 B.C.
Middle Helladic II.....	1800-1600 B.C.
Middle Helladic III.....	1600-1500 B.C. (LH I)
Middle Helladic IV.....	1500-1400 B.C. (LH II)
LATE HELLADIC PERIOD.....	1400-1100 B.C. (LH III)

¹ *Korakou*, p. 123.

² V. Fimmen, *Die kretisch-mykenische Kultur* (1921), pp. 212 sq., *Synchronistische Tabelle*, pp. 210-211; H. R. Hall, *The Ancient History of the Near East* (4th ed., 1919), pp. 36 sq. These two most useful and valuable books will hereafter be referred to as Fimmen and Hall, respectively.

II. THE NEOLITHIC PERIOD (? TO *circa* 2500 B.C.)

As yet no detailed report on the Neolithic finds in the Peloponnesos has appeared, but Dr. A. L. Walker will soon publish the results of her excavations at Corinth, where she has found a considerable quantity of Neolithic ware.¹ And at Gonia, one of the "prehistoric" sites which Dr. Blegen discovered in the Korinthia, he has found Neolithic sherds that are closely related to Thessalian pottery of the Second Period.²

These Neolithic people in the Korinthia may have belonged to a Northern race, and these few settlements may have been outposts or border settlements of a Northern civilization. However, future excavations may bring to light Neolithic settlements scattered throughout the Peloponnesos. Mr. H. R. Hall is of the opinion that the Neolithic and the Bronze Age peoples of Hellas were of one and the same race;³ and indeed the two Neolithic sites that I know of were succeeded by Early Helladic settlements. But it seems to me that the comparatively late appearance or use of bronze on the mainland militates against this theory. If the Neolithic Peloponnesians were of the same race as the Minoans, or as the Kykladic people, it is hard to understand how the people on the mainland could have remained in a state of Neolithic culture down to so late a period (*circa* 2500 B.C.), when their kinsmen on the neighboring islands had learned the art of making bronze about half a millennium previously.

But, until we learn more about the pottery of these Neolithic people, and about the stratification of the Neolithic deposits, their racial and cultural affinities must remain indeterminate. Personally, I am inclined to class the Neolithic Peloponnesians with the peoples belonging to a Northern culture group.⁴

¹ Dr. Walker has kindly given me permission to mention this.

² Since this was written, Dr. Blegen has discovered a Neolithic site at Hagioritika near Tripolis, where sherds resembling the Neolithic ware of the Second Thessalian Period were found: *v. note* in *A. J. A.*, 1922, p. 469; for Gonia, *v. Carl W. Blegen, Corinth in Prehistoric Times*, in *A. J. A.*, 1920, pp. 1 sq., esp. p. 7. Near Megalopolis Tsountas has found four stone implements which he thinks belong to the Neolithic Age ('Εφ. 'Αρχ., 1901, col. 85-90).

³ Hall, p. 31. Of course, he is speaking of the entire Aegean basin as "Greece."

⁴ For a discussion of the Northern culture groups, *v. Dr. Ida C. Thallon, Some Balkan and Danubian Connexions of Troy*, in *J. H. S.*, 1919, pp. 185 sq.

III. THE EARLY HELLADIC PERIOD (*circa* 2500 TO 2000 B.C.)

I. *The Archaeological Evidence*

(a) Pottery. — This first period of the Bronze Age is characterized by hand-made pottery and the use of lustrous paint for the decoration of the painted pottery; also quite characteristic of this period are several shapes, notably the sauce-boat, askos and shallow bowl or saucer.

The pottery has been grouped into three chronological periods.¹ The first part of the period is characterized by unpainted and polished pottery, some of which has incised decoration resembling that of Early Kykladic pottery. Then comes the Glazed ware (ware decorated with lustrous paint), once called "Urfirnis"; and in the latter part of the Early Helladic Period both the Glazed and the Patterned wares appear.

It is to be noted that the Early Helladic pottery has close affinities with the contemporary Kykladic wares. In particular, the sauce-boat and shallow bowl are popular shapes in both cultures; and in addition to the use of lustrous paint in both places and the apparently synchronous termination of its use in favor of Mattpaint, there are other parallels. That trade-intercourse, or communication by sea, existed in the Early Helladic Period between the Peloponnesos and the Kyklades is evidenced by the great quantity of Melian obsidian found at many Early Helladic sites on the mainland. There are also several objects, found on the mainland, which are very probably imports from the Kyklades — for example, the marble idol found at Zygouries.²

(b) Houses. — One of the most important results of the American School's excavations at Zygouries in 1921 was the discovery of the ground-plan and general structure of the Early Helladic house. Previously only portions of house-walls of this early period had been

¹ The best description of Early Helladic pottery is to be found in *Korakou*, pp. 4 sq.; v. also pp. 110 sq.

² This idol (the head and legs are missing) is a typical Kykladic marble figurine, and, with the exception of one from Sounion (recently noted by Dr. Holland at Athens), the only one that I know of found on the mainland.

found. The immense round building discovered at Tiryns,¹ which is apparently to be dated in the Early Helladic Period, is by reason of its great dimensions (its diameter is over 30 metres) obviously an exceptional building, and should not be taken as a typical example of the dwelling of this early period.

At Zygouries several complete ground-plans were made out.² The Early Helladic house was rectangular in plan, with a tendency toward squareness, and it was obviously flat-roofed. The houses, usually consisting of two rooms placed one behind the other, were clustered together and separated only by very narrow, crooked alleys.

As yet no Early Kykladic house-plan has been definitely ascertained, but the house was very probably rectangular in form and flat-roofed, as were the Kykladic houses of the subsequent period (*e.g.* Phylakopi, Second City) and the contemporary Early Minoan houses in Krete. The Early Kykladic settlements, if we may judge by the succeeding settlements on the same sites, apparently resembled the village at Zygouries, in that the houses were crowded together, and the streets were mere alleys, irregular and narrow.³

(c) Graves. — Since no Early Helladic cemeteries have been found, it is impossible to compare the burial customs of the Early Helladic and the Early Kykladic civilizations. The rock-cut chamber-tombs found at Korinth in 1896,⁴ which contained Early Helladic burials, are practically shaft-graves, — the bodies were placed in chambers opening on a shaft, — and the shaft-grave was not a very frequent form of grave in the Bronze Age. So this isolated Korinthian example cannot be used to disprove a similarity of culture between the Early Helladic people and their contemporaries on the Islands. For, although the typical Kykladic grave is the cist-grave, yet rock-cut chamber-tombs have been found in the Kyklades and in Krete, and

¹ Fimmen, p. 40; *Ath. Mitt.*, 1913, pp. 78 sq. (K. Müller), and pp. 329 sq. (Dragendorff); Karo, *Führer durch die Ruinen von Tiryns*, pp. 7 sq. I have not been able to consult the latter, and cannot give the exact reference.

² *Art and Arch.*, XIII (1922), pp. 211–212 (by Dr. Blegen).

³ *Excavations at Phylakopi in Melos* (1904), pp. 35 sq.

⁴ *Pre-Mycenaean Graves in Corinth*, in *A. J. A.*, 1897, pp. 313 sq. Cf. Fimmen, p. 59, and Dussaud, *Les civilisations préhelléniques dans le bassin de la Mer Égée* (1st ed., 1910), p. 25, fig. 11 — grave at Zafer Papoura; and Fimmen, p. 104, Abb. 92 — grave in Kypros.

cist-graves have been discovered at several sites on the mainland. Though they are of later date, they may be survivals of an earlier burial custom at these places.¹ It will be necessary to discover an Early Helladic cemetery before a satisfactory comparison of the two cultures in respect to burial customs may be attempted. The most that can be said now is that in both places the dead appear to have been buried in special places away from the houses. In this connection should be mentioned the Zygouries marble idol. This may have come from an Early Helladic grave; and if so, we have another link for connecting the two civilizations, for the idol is a typical Kykladic grave figurine.²

From a study of the archaeological evidence it becomes clear that the Early Helladic civilization of the mainland of Hellas was closely related to that of the Aegean Islands in the Early Kykladic Period, and that in this period trade-intercourse existed between the Kyklades and the mainland.

2. Linguistic Evidence

The earliest dialect in the Peloponnesos of which we have any knowledge is that which is represented by the place-names with $\nu\theta$ and $\varsigma\varsigma$ which are paralleled by names with $\nu\delta$ and $\varsigma\varsigma$ in Asia Minor. Kretschmer, on philological grounds, has argued that a non-Indo-European race, akin to the non-Indo-European Lykians of Asia Minor, once inhabited the Aegean Islands and the mainland of Hellas.³ Therefore, we may conclude that the people who left traces in the place-names, Tirynthos, Korinthos, Saminthos, and Erymanthos, belonged to an Anatolian race of non-Indo-European stock, and were akin to the Lykians of Asia Minor. To this same race probably belonged the Early Kykladic peoples and also, by intermixture, the Early Minoans of Krete. Now this people with Anatolian affinities preceded the first of Hellenic or Indo-European stock, and the latter entered the Peloponnesos in the Middle Helladic Period.⁴ Therefore, we may

¹ Fimmen, pp. 60 sq., esp. p. 63 (kinds of tombs and where they are found).

² V. n. 2 on p. 8, *supra*; cf. 'Εφ. 'Αρχ., 1898, p. 176; and Fimmen, pp. 13 and 14 for references.

³ Paul Kretschmer, *Einleitung in die Geschichte der griechischen Sprache* (1896), pp. 401 sq.; or in Gercke-Norden, I, pp. 146 sq.

⁴ V. p. 19 *infra*. This is exclusive of the Neolithic people who may have been Indo-Europeans.

further conclude that this people of Anatolian stock on the mainland were the Early Helladic Peloponnesians, or, conversely, that the people who inhabited the Peloponnesos in the Early Helladic Period were of non-Indo-European stock and belonged to an Anatolian race, akin to the Lykians of Asia Minor.

3. *The Traditions*

But we are not dependent upon archaeological and dialectic evidence for the theory that a people of Anatolian race once occupied the Peloponnesos. There was a generally accepted tradition that foreigners came and settled in the Peloponnesos in early times. Pelops came to the Argolid, or Elis, from Phrygia in Asia Minor; the Kyklopes, from Lykia in Asia Minor, built the walls of Tiryns; and Karians, or Leleges, settled in Lakonia, Messenia, and several places in the Argolid, not to mention their reputed settlements outside of the Peloponnesos.¹

In the same category are the legends concerning Kadmos, who is said to have come to Thebes from Egypt or Phoinikia, and about Minos and the tribute which he exacted annually from the Athenians. It has been suggested that in these traditions we should read Krete in place of Egypt or Phoinikia.² And those who believe in a Kretan conquest of southern Hellas, or parts thereof, may use these emended traditions in support of their theory. The Early Minoans were quite probably akin to this Anatolian folk, and the spreading of this people to the Peloponnesos might well come to be called a conquest. However, it is the Anatolian people who play the great part in the traditions, and it is generally believed that these Anatolians, popularly known as Karians, did spread out over the Aegean Islands to Krete

¹ The Leleges may have been the early Asia Minor people who in time came to be subjected to, or at least grouped with, the Karians. For Karians and Leleges in Hellas: Eduard Meyer, *Gesch. des Alt.*, I (1921), 2, sec. 506, pp. 765 sq.; Beloch, *Griechische Geschichte*, I, 1, pp. 75 and 96 sq.; How and Wells on Herodotos, I, 171; *et al.* Cf. tradition of the Karian thalassocracy; Diodoros, 5, 84, and J. L. Myres, *On the List of Thalassocracies in Eusebius*, in *J. H. S.*, 1906, pp. 84 sq., esp. pp. 107-109.

² Hall, p. 60, n. 2. But the Kadmeans were probably an early Boiotian people; Kadmos is a Hellenic name derived from the hill Kadmea; so says Fimmen, *Besiedlung Böotiens bis in die frühgriechische Zeit*, in *Neue Jhrb.*, 1912, p. 536.

and to the Helladic mainland in very early times.¹ Thus, the traditions are, on the whole, in accord with the archaeological and the linguistic evidence.

For these Early Helladic inhabitants of the Peloponnesos I have adopted the name "Aigaïans." The reason for the choice of this name I have given elsewhere.² "Anatolians" is too extensive a term, as it may include the early inhabitants of many lands, *e.g.*, Asia Minor, Krete, and the Kyklades; and there is the same objection to the name "Mediterraneans." Karians has a local significance, and, besides, has been used loosely to include Anatolians. "Early Helladic people" is too cumbersome a term, and "Aegeans" has been used in such various ways that it may designate the peoples of the Aegean lands of any or all periods. But "Aigaïans" can be made to connote just one thing, namely, the inhabitants of the Peloponnesos in the Early Helladic Period.

IV. THE "MINYAN" MIGRATION

That the Early Helladic civilization was brought to an abrupt and sudden end by an invading people from the North is clearly shown by the archaeological evidence from several sites. In fact, there are five or, possibly, six points of evidence in support of the theory (apart from the dialectic evidence).

(1) The excavations at Korakou showed that the Early Helladic stratum was separated from the succeeding Middle Helladic stratum by a well-marked layer of ash, which seemed to extend over the whole site. This led Dr. Blegen to believe that the last Early Helladic settlement there was destroyed in a general conflagration.³

At Zygouries also, there are traces of a conflagration at the end of the Early Helladic Period. On the floor of one or two of the houses

¹ In Early Helladic times; cf. Cavaignac, *Histoire de l'Antiquité*, I, 1, p. 33: "Sur les populations contemporaines de Troie . . . les Cariens, qui à cette époque devaient habiter les Cyclades et peut-être la Grèce, étaient certainement un peuple asianique et non indo-européen."

² In my *Prehistoric Aigina*, ch. II; but *v.* p. 53, *infra*. This Aigaïos, or at least the place-names with the root 'Aig-' seem to precede the Posoidan-worshippers, and so did the Anatolian folk who spread out over the *Αιγαῖον Πέλαγος*. Hence, "Aigaian" is an apposite label.

³ *Korakou*, p. 2.

large sun-dried bricks were found, which had fallen from the walls and had been baked hard by the fire that seems to have destroyed the houses.

(2) Several Early Helladic sites were apparently destroyed at the end of this period, for they were never reoccupied in the succeeding ages. Four such sites are the hill of Hagios Gerasimos, Yiriza, Kenchreai, and a settlement at the Isthmos.¹

(3) The end of the Early Helladic Period is marked by the sudden cessation of the typical hand-made Early Helladic pottery. The use of lustrous paint for decoration suddenly terminates. The typical Early Helladic shape, the sauce-boat,² disappears with the period, never to reappear, and the Patterned ware which developed in the latter part of the Early Helladic Period ceases to be made.

(4) But the most striking indication of a change is the sudden, unheralded appearance of wheel-made Gray Minyan ware. This wonderful ware has been fully described by Forsdyke and Childe. But since it plays so conspicuous a part in this migration, I shall give a description of Gray Minyan, which I have taken from Dr. Blegen's *Korakou*.³

"It is a wheel-made pottery of high technical excellence. The clay is extremely fine, and of a characteristic color which varies in different vessels from a very light to a very dark gray. This color is in most cases uniform quite through the biscuit, as well as on the surface, and is probably due to the effect of some particular process of firing on the mineral elements in the clay. The surface is smoothly polished and has a characteristic soapy feel. The fabric is usually thin. The shapes of the vases are graceful and have an unmistakable appearance of imitations of metal prototypes. The great bulk of Gray Minyan is undecorated, but a number of sherds were found which have a simple incised design in curved or wavy lines."

It seems most probable that the origin of this ware is to be found in

¹ Blegen, *A. J. A.*, 1920, pp. 4, 6, and 8.

² Illustrations of a sauce-boat: *Korakou*, fig. 6, Pl. I; *B. S. A.*, XXII, Pl. VI and VII; Fimmen, Abb. 132.

³ *Korakou*, p. 15; v. Forsdyke, *The Pottery called Minyan Ware*, in *J. H. S.*, 1914, pp. 126 sq.; Childe, *On the Date and Origin of Minyan Ware*, in *J. H. S.*, 1915, pp. 196 sq. Illustrations: *Korakou*, figs. 18-20; *J. H. S.*, l.c.; Fimmen, Abb. 140-141; Wace and Thompson, *Prehistoric Thessaly*, fig. 135.

Phokis.¹ Its sudden appearance in such a highly developed state, its immediate widespread use, and its position as the first-class ware of the First Middle Helladic Period, all combine to force us to believe that this Minyan ware was brought down from the North by an invasion.² This invasion I have termed the "Minyan" Migration, because of the conspicuous part that Gray Minyan ware played in the event.

(5) Another piece of evidence for an invasion of Northerners at the beginning of the Middle Helladic Period is to be found in the apparent change in the architecture, or house-construction, at this time. The Early Helladic house, rectangular or square in plan and flat-roofed, appears to have been superseded by the curvilinear house with a hoop-roof.³ Houses of this curvilinear plan (they may be oval, in the form of a horse-shoe, or of a hair-pin) have been found, it is true, at only a few sites; but if, as seems probable, the curvilinear houses had hoop-roofs, they would naturally be of a rather flimsy construction; and not requiring much of a foundation, they would often leave no traces. Besides, the curvilinear house appears to have been the forerunner of the house of the megaron-type, and hence it is natural that it should have left but few traces. But examples have been found in the Peloponnesos. At Korakou there is a "hair-pin," or apsidal, house, which is dated by the pottery in the Middle Helladic Period.⁴ An oval house has been discovered on the Citadel at Tiryns,⁵ and several

¹ The occurrence of so many large Gray Minyan storage jars at Drachmani and Hagia Marina in Phokis precludes the probability that they are imports; one would not expect large, bulky, ordinary undecorated storage vessels to be imported into the interior of Phokis. Cf. Childe, *l.c.*; Soteriades, *Fouilles préhistoriques en Phocide*, in *Rev. Études Gr.*, 1912, pp. 253 sq.; Dawkins and Droop, in *B. S. A.*, XVII, p. 16: "It is reasonable to believe that it was made in Phokis."

² While I believe that the Middle Helladic Phokians and Orchomenians (Boiotian) were of the same stock and of the same dialect-stratum as the "Minyans" of the Peloponnesos, yet all the peoples who made up the "Minyan Migration" obviously could not have come from Phokis alone. Hence, I have used the general term North, which includes Phokis and neighboring districts. The process of manufacture seems to have been brought in, and not just the vases; some of the finest Aiginetan Minyan ware, for instance, was certainly of local make.

³ Dr. L. B. Holland has traced the development of the curvilinear house in *Primitive Aegean Roofs*, *A. J. A.*, 1920, pp. 323 sq., esp. pp. 324-325.

⁴ House "F," *Korakou*, pp. 76 sq., fig. 110.

⁵ *Ath. Mitt.*, 1913, pp. 85 sq.; Fimmen, p. 43, and Abb. 33.

“horseshoe” and “hair-pin” houses have been found at Olympia.¹ Outside of the Peloponnesos the curvilinear houses at Thermon and Orchomenos are probably the best-known examples.

(6) The burial customs of the Early and Middle Helladic Periods cannot as yet be very well compared, since so little is known about Early Helladic burials. The only Early Helladic graves so far discovered are the rock-cut chamber-tombs at Korinth,² and these, as has been suggested above, may not be typical of the period. As evidence of a negative character, it may be mentioned that there have been found no traces of Early Helladic infant burials under the floor of an Early Helladic house.

In the Middle Helladic Period it was a common practice to bury infants and small children under the floor of the house.³ Sometimes the infant was placed in a pithos (usually a broken one that could be spared), and sometimes on a bed of stones, with or without a covering of broken pieces of pithoi. The body usually lay on its side in the “contract” position, and vases were not buried with the body. The adults were probably buried in cemeteries and, if we may judge by the Sixth Shaft-Grave at Mykenai, in rock-cut graves. But this grave comes at about the middle of Middle Helladic II, and the adult burial at Zygouries,⁴ which is to be dated in Middle Helladic I, is not in a shaft-grave, but the body lay (on its side and in the “contract” position) inside a simple, irregular ring of stones, possibly in or near a house. Then too, Stais found at Aigina the bones of an adult in the wall of a Middle Helladic house.⁵ But more evidence will have to be discovered before adequate comparisons of burial customs may be

¹ Holland, *l. c.*, I, p. 326, fig. 2. Fimmen, p. 44; “Datiert hat man die Bauten in mykenische Zeit”; i.e., according to Fimmen, 1700–1250; but obviously before 1400, since at the most only one Late Helladic sherd has been found at Olympia. Therefore, the houses are to be dated in the Middle Helladic Period.

² A. J. A., 1897, pp. 313 sq. Vases were found with the bodies.

³ Dawkins and Droop, *Intramural Tombs of Infants*, in *B. S. A.*, XVII, pp. 6 sq.; *Korakou*, pp. 100 sq. Cf. Plato, *Minos*, 315 D.

⁴ With the skeleton, which was lying on its right side in the “contract” position, were found two Matt-painted vases and various objects; *v. Art and Arch.*, XIII (1922), p. 215.

⁵ Stais, *Ἐφ. Ἀρχ.*, 1895, pp. 235 sq. The houses in which he found the infant burials and the adult osteothekē are said by him to be older than the Mykenaian Age, and I have found a quantity of Gray Minyan around these house-walls.

made. The most that can be said now is that the Early and the Middle Helladic peoples seem to have differed in respect to the intramural infant burials.

Along with Gray Minyan another new ware appears in the First Middle Helladic Period, namely, Mattpainted ware. This ware was usually hand-made and of coarse clay, and in Middle Helladic I it was employed only for large and coarse vessels. The commonest variety has a biscuit of yellowish-green clay with the decoration in Mattpaint, black, blackish-brown, or brick-red in color. At first the designs were merely angular, but, later, curvilinear designs also were used.¹

Although this ware does appear suddenly, yet it cannot be used as evidence for an incursion of a new people from the North. In view of the fact that comparatively few sherds have been found in North and Central Hellas, its provenance cannot be sought in these regions. The Mattpaint technique is probably a local development, originating in the northeastern part of the Peloponnesos, or in the Kyklades. The Mattpainted pithoi which have been found in such quantities at Aigina, Argos and Korakou are of the same shape as the low-bellied Early Kykladic pithoi of Melos; and it has been suggested that the Mattpaint is a Melian invention, induced by the necessity of finding a substitute for the lustrous paint which the Melian clay could not take well on account of its coarseness and porosity. But the Early Kykladic pithoi may have merely suggested the shape to the mainlanders — they were decorated with lustrous paint — and the Mattpaint may have originated in Hellas. Mr. Edgar, one of the excavators of Phylakopi, has expressed the opinion that the Mattpaint technique came to Melos from the Peloponnesos.²

Now, one of the greatest centres for Mattpainted ware is the island of Aigina, and the characteristic clay of this ware is the yellowish-green, coarse clay which is found on this island. The unslipped vessels of this clay would take the lustrous paint as poorly as the pottery of Melian clay, and Aigina may well have been the home of Matt-

¹ Description of the three groups of Mattpainted ware in *Korakou*, pp. 19 sq.; and in *B. S. A.*, XXII, pp. 183 sq. For this, as for all wares, Fimmen is the best book for the *Fundorte* and references.

² *Excavations at Phylakopi*, p. 93.

paint. Fimmen has even named the Mattpainted ware (Blegen's Group I), "Äginaware."¹ Of course, the Mattpaint technique may have developed in the two localities, Aigina and Melos, independently, but until we have more evidence on the question of the provenance of this technique, it seems to me more probable that the island of Aigina was the home of Mattpainted ware. Although the appearance of this new pottery does not indicate the invasion of a new people from the North, yet it does illustrate the break in the cultural sequence between the Early and Middle Helladic Periods, and should be mentioned here.

To summarize the argument for an invasion at the end of the Early Helladic Period, I shall briefly restate the evidence.

(1) There was a layer of ash between the Early and Middle Helladic strata at Korakou, and Zygouries also showed signs of a fire at the end of the Early Helladic Period.

(2) Several Early Helladic sites were destroyed and never re-inhabited.

(3) There is the cessation of the typical hand-made Early Helladic pottery, both shapes and the use of lustrous paint disappearing.

(4) There is the sudden appearance of wheel-made Gray Minyan pottery. The appearance of Mattpainted pottery supplements the other evidence. The Gray Minyan, being synchronous with Kretan Kamares ware and with the XIIth Dynasty in Egypt, dates this invasion roughly *circa* 2000 B.C.

(5) Change in the type of house. The curvilinear hoop-roofed house supersedes the Early Helladic rectangular house with a flat roof. The megaron-type of house enters the Peloponnesos.

(6) Possible change in the burial customs.

There seems, therefore, to be sufficient evidence for the acceptance of the theory that a hostile invasion of a Northern people brought about the subjection of the Early Helladic people and the end of the Early Helladic civilization in the Peloponnesos *circa* 2000 B.C. It is interesting to note that Eduard Meyer,² by inference from what we may term indirect evidence, concludes that the first people of Indo-

¹ Fimmen, p. 77.

² *Geschichte des Altertums*, I (1921), 2, sec. 527, p. 808.

European stock to enter the southern part of the Balkan Peninsula must have arrived not later than approximately 2000 B.C. And Beloch, by a similar process of reasoning, arrives at the same approximate date for this event.¹ So the new archaeological evidence has, to a surprising degree, confirmed the conjectures of Meyer, Beloch, and other historians, and, it seems to me, practically establishes it as a fact that a migration into the Peloponnesos of a new people from the North occurred at about the end of the Third Millennium, that is, *circa* 2000 B.C.

This migration I have termed the "Minyan" Migration because of the conspicuous part played by the Gray Minyan ware in the event, and because of the widespread use that this ware had in the first two centuries of the newcomers' régime, as well as because of the influence that it had on the mainland pottery down to the end of the Bronze Age. The typical Minyan shapes persisted long after the Gray Minyan technique had fallen into disuse, or had gone out of style. It was the Minyan element that made Mykenaian pottery different from the Minoan.²

V. THE MIDDLE HELLADIC PERIOD (*circa* 2000 TO 1400 B.C.)

1. *The Dialect*

There are three well-defined strata of dialects in the Peloponnesos: first, the non-Indo-European; second, the Indo-European "Arkadian," so named from the locality where it existed down into classical times; and lastly, the Dorian dialect.³

There is no question about the chronological order of these dialects, nor of the peoples who spoke them. That the "Arkadian" dialect represents an earlier stratum of people than the Dorian, is clearly shown by the geographical position of the people who spoke "Arkadian" in classical times. The island of Kypros, as the great similarity of its dialect to the "Arkadian" shows, was colonized by this "Arkadian-tongued" people, who, therefore, must have been at one time on the sea-coast. But in "historical" times we find them pushed back

¹ Beloch, I, 1, pp. 71 and 92.

² Cf. *Korakou*, pp. 44 and 117; *B. S. A.*, 1916-1918, p. 189.

³ Beloch, I, 1, pp. 87 sq.; 2, pp. 88 sq. Kretschmer distinguishes four strata of dialects (*Gercke-Norden*, I, pp. 146 sq.).

from the coast into the interior and surrounded by the obviously later arrivals who spoke Doric.¹

The "Arkadian" dialect, therefore, represents the second stratum of people in the Peloponnesos.² We have seen that the first dialect (of which we have any trace) was spoken by the non-Indo-European "Aigaians" who inhabited the Peloponnesos in the Early Helladic Period. These "Aigaians" were conquered and succeeded by invaders from the North, who entered the Peloponnesos at the beginning of the Middle Helladic Period. Now, if these latter people colonized Kypros in Middle Helladic IV or in the Late Helladic Period (Wace and Blegen's LH II or III), that is, *circa* 1400 B.C.,³ they must have been in the Peloponnesos for some time before this date. But the only apparent 'break' or evidence of an invasion previous to the Late Helladic Period is the "Minyan" migration of *circa* 2000 B.C. Therefore, the dialect of this Middle Helladic people was the so-called "Arkadian," an Indo-European dialect, and so, this Middle Helladic people whom I shall label "Minyans" represents the first people of "Hellenic" or Indo-European stock to enter the Peloponnesos.⁴

I have labelled this Middle Helladic people and their civilization "Minyan," taking the name from the Gray Minyan ware which played so conspicuous a part in their invasion, and also in the subsequent ceramic history of the mainland (*v.* end of Section IV, p. 18, *supra*).

The objection has been raised that these "Minyans" would be confused with the Minyai whom tradition locates in Boiotian Orchomenos. But this confusion can do no harm; in fact, it may even be desirable, so long as the pre-Mykenaian or Middle Helladic inhabitants of Orchomenos are thought of as Minyai. I think that the civilization and people of Central and Southern Hellas in the Middle Helladic Period were essentially the same.

¹ *V.* dialect maps: at back of Beloch, I, 2, and Pl. V in Buck, *Greek Dialects* (1910).

² That is, the second stratum of dialects of which we have any trace.

³ The island appears to have been colonized in the fourteenth century; *v.* Nilsson, in *Gött. Gel. Anz.*, 1912, pp. 526 sq. Hall (p. 66) says that a colony appears there suddenly *circa* 1400, but from Krete. It is interesting to note the tradition that Arkadians returning from Troy were carried by a storm to Kypros (Paus. 8, 5, 2).

⁴ Excepting, of course, the Neolithic people who may have been Indo-Europeans.

Furthermore, to offset the objection, I may add that there is some justification for labelling the "Arkadian-tongued" people "Minyans." Not only can evidence be adduced for the presence of Minyai in the Peloponnesos, but also for their identity or kinship with the Arkadians and Triphylians. And the traditions are apparently independent of the Argonaut saga. In the *Iliad* mention is made of a river Minyeios near Triphylian Pylos. In the *Odyssey* we are told that Neleus of Pylos had married Chloris, the daughter of Amphion the king of Minyan Orchomenos. Herodotos tells of six towns (Lepreon, Pyrgos, etc.) that were founded by the Minyai, but subsequently taken by the Eleians. Strabo says that Minyai came to Triphylia with Chloris from Minyan Orchomenos, and in another passage shows that Minyai were associated with, if not identified with, Arkadians and Triphylians.¹

However little historicity there may be in this testimony, however much there may be that is pure invention on the part of the logographoi, at least there is evidence, albeit legendary, for the presence of Minyai in the Peloponnesos and for the association of Minyai with towns in Elis and Triphylia. And in the case of a label, it is permissible to avail one's self of even pure myth. Therefore, it seems to me, the objection may be set aside. Anyone having in mind the Middle Helladic pottery will have no difficulty in seeing in the label "Minyans" that people who introduced the well-known Minyan ware into the Peloponnesos; and the mind, with the aid of the alliteration, will readily associate "Minyans," Middle Helladic, and Minyan ware.

2. Traditions and Cults

According to the tradition, the Dorians found the Peloponnesos already occupied by a people of their own race, that is, a people of "Hellenic" or Indo-European stock.² But, aside from the fact that

¹ *Iliad*, II, 722; *Odyssey*, II, 281; Herodotos, 4, 148; Strabo, 8, 337, 346, 347. These people have been called "Arkadians," because they spoke the so-called "Arkadian" dialect. But it seems to me that the name "Minyan" is superior to "Arkadian," for the latter has a local significance, and hence is undesirable for a label; and, worse still, the people would be receiving their name from a locality where they did not flourish in Middle Helladic times, but whither some of them had been driven by their conquerors.

² The Herakleidai were supposed to have been in the Peloponnesos before the Dorians. V. Beloch, I, 1, p. 92.

they were Indo-Europeans, tradition gives us scanty evidence for the pre-Dorian people in the Peloponnesos, and we must turn to the study of cults for more light on this period.

The chief deity, or certainly a very important one, of the "Minyans" was Poseidon, or, as they called him in their dialect, "Posoidan." The frequent occurrence of the cult of Posoidan in Arkadia shows this;¹ and it is to be noted that the heraldic device on the shields of the Mantineans was the trident.²

Then too, the pre-Dorian stratum in Lakonia is represented by the cult of "Pohoidan," the Lakonian form for Posoidan. There is abundant evidence of this cult of Posoidan in Lakonia,³ a centre of which appears to have been in the remote southern end of Lakonia on Cape Tainaron. That it was a pre-Dorian cult is indicated by the fact that the form of the name Pohoidan which occurs in Lakonian inscriptions is obviously the Lakonian corruption of the earlier, and hence pre-Dorian name, Posoidan. Besides, the geographical position of the cult-centre (or hieron of Posoidan) in the extreme southern part of the almost inaccessible mountainous peninsula of Mani, as well as the fact that this cult was supposed to be primarily a cult of the Helots,⁴ both show that the cult of Posoidan belonged to the pre-Dorian stratum. Hence, we may conclude that the god Posoidan was the chief or most popular deity of the "Minyans."

That the Posoidan-worshipping "Minyans" occupied the Peloponnesos prior to the Dorian-tongued people is further indicated by the legends that Poseidon contended with Zeus for the possession of Aigina,⁵ and with Hera for Argos,⁶ but unsuccessfully in each case.

¹ Farnell, *Cults of the Greek States*, IV, pp. 86-87, n. 64, for references; IG V fasc. 2, nr. 46, 47, etc.; Paus., 8, 10, 2 (a temple of Poseidon Hippios near Mantinea).

² Bacchylides, frg. 6 (Jebb). One of the tribes of the Mantineans was named Posoidaia, IG V fasc. 2, nr. 271.

³ IG V fasc. 1, pp. 230 sq., nr. 1228-1232; Strabo, 8, 363; Paus., 3, 25, 4 and cf. 3, 12, 5.

⁴ Eupolis, frg. 140 (Kock); Schol. to Aristophanes, *Acharnians* 509, and *Knights* 1222; v. Farnell, *l.c.*, p. 41. It is uncertain, but I do not believe that the Helots were Dorians; v. Beloch, I, 1, p. 205, n. 2.

⁵ Pythainetos, in Schol. to Pind., *Isth.*, 8, 92.

⁶ Plutarch, *Quaest. Conviv.*, 9, 6, p. 741 (*Moralia*, vol. IV, Tb. 1892). Hera was an important Argive deity, even in the Mykenaian Age; cf. *Iliad*, 4, 52.

Now Zeus and Hera are deities of the "Achaïans." So in these legends we may see the reflection of the unsuccessful resistance offered by the Posoidan-worshipping "Minyans" to the incursion of the "Achaïans," who eventually drove the "Minyans" back from the sea-coast into the interior, or forced them to seek new homes across the sea. The priority of the "Minyans" in the Peloponnesos is also suggested by the numerous traditions to the effect that the Arkadians were autochthonous, and that Arkadia was once called Pelasgia, etc.¹

I think it more than probable that the "Minyans" identified their own Posoidan, possibly a "Minyan" marine deity, with the sea-god of the "Aigaians," whom they quite probably had assimilated. As I have suggested elsewhere,² the chief deity of the Early Helladic people seems to have been a marine deity, possibly named "Aigaïos." This "Aigaïos" was later identified with, and supplanted by, Posoidan, but not until he had given his name to many places, for example, Aigiai, Aigina, Aigira, etc., all of which are situated on the sea and usually connected with the god Poseidon. A remark of Professor Prentice has suggested the idea that the peculiar association of the horse with the sea-god Poseidon may be explained by the supposition that the "Minyan" Posoidan originally was not a marine deity, or at least had some non-marine associations before he became identified with (and perhaps lost his identity in) the sea-god of the "Aigaians."³

The people who spoke the "Arkadian" dialect, then, worshipped the god Posoidan, and, as we learn from the study of dialects and cults, these people were post-"Aigaian" but pre-Dorian, and hence belong to the Middle Helladic Period.

3. *The Middle Helladic Sub-Periods*

The Middle Helladic Period may be subdivided into four sub-periods or divisions. Although these divisions correspond with changes of level of habitation at Korakou, yet they are primarily stylistic.

¹ Hellanikos, frg. 77 (FHG I, p. 55); Herodotos, 8, 73; Paus., 5, 1, 2. The Arkadians are included in my "Minyans"; v. n. 1, p. 20, *supra*.

² V. n. 2, p. 12.

³ That the inland Poseidon Hippios still kept up his connections with the sea is shown by the legend that a wave of the sea appeared in the hieron (near Mantinea); Paus., 8, 10, 4.

There is no evidence of a cultural 'break,' or of a change of race in the Peloponnesos between the approximate dates 2000 and 1400 B.C. Hence this classification will be found to be applicable to most sites or districts of the Peloponnesos.

(1) Middle Helladic I (*circa* 2000 to 1800 B.C.)

This is the period *par excellence* of Gray Minyan ware. Side by side with Gray Minyan, Mattpainted ware was used; but whereas the former was used for the finer and better vessels, the Mattpainted ware was used for large and coarse vessels, such as pithoi, storage-jars, etc. At first the decoration consisted of angular designs in Mattpaint, but soon curvilinear designs began to be used. In addition to true Gray Minyan, varieties or local imitations of this ware were made. In the Peloponnesos Gray Minyan ware has been found at half a dozen sites in the Korinthia, at Zygouries, Mykenai, Tiryns, Argos, Midea, Aigina and Olympia; and Mattpainted ware (Blegen's Group I) at Aigina, in the Korinthia and the Argolid, in Lakonia and Elis.¹

(2) Middle Helladic II (*circa* 1800 to 1600 B.C.)

In this period the use of Gray Minyan (in some districts at least) falls off considerably, and its place is taken by Yellow Minyan. The coarse vessels continue to be made of Mattpainted ware (Group I), while the finer vessels are made of Yellow and Gray Minyan, and of the finer fabric of Mattpainted ware (Group II).² But the new element, which really differentiates Middle Helladic II from the previous period, is the reappearance on the mainland of decoration in lustrous paint on what Dr. Blegen has called "Mainland Ware corresponding to Fabrics of the Third Middle Minoan Period."³ This ware is the mainland version of the Kretan light-on-dark and dark-on-light styles of Middle Minoan III, and it represents the beginning of the infiltration of Minoan influence into the Helladic mainland culture. It is to be noted that this Patterned ware with lustrous paint came in gradually, and that comparatively few sherds have been found in the Second Middle Helladic stratum.

¹ Fimmen, p. 79, and *passim* in works cited in n. 3, p. 13.

² Description of the pottery of this and the preceding period in *Korakou*, pp. 15 sq. Of course, in all periods unpainted coarse ware was used for cooking vessels.

³ The "D" wares of the Middle Helladic Period; *Korakou*, pp. 32 sq.

Another important feature in Middle Helladic II is the Sixth Shaft-Grave at Mykenai which received its first interment about the middle of this second sub-period.¹ It is to be noted that the six Shaft-Graves represent an unbroken series from the middle of Middle Helladic II down to Middle Helladic IV (LH II). So those who would believe in a Kretan conquest in the Peloponnesos will have to date it before the middle of Middle Helladic II. A Kretan conquest at the end of Middle Helladic IV (*circa* 1400 B.C.) is out of the question; for the invasion which brought about the destruction of the palace at Knossos at the end of the fifteenth century undoubtedly came from the North, and so drove the Kretans in every direction but to the North. And there seems to be little evidence of a Kretan conquest in the Peloponnesos in the previous periods.²

The infiltration of Minoan influence was gradual and the result of natural causes. One would expect a higher culture to affect the lower culture of a neighboring region, especially if the two peoples began to have closer relations with each other. The Shaft-Graves form a conspicuous feature, by reason of their contents and not because they are shaft-graves, and the richness of their contents may reflect the prosperity which the Middle Helladic people had finally acquired, probably through transmarine trade. If the "Minyans" were the ones who overran Krete and destroyed the first palace at Knossos (in MM III or LM I),³ this would account in part for the increase of Minoan influence, as evidenced by the pottery, at the beginning of Middle Helladic III.

The period that follows Middle Helladic II (and is more or less synchronous with the beginning of the XVIIIth Dynasty in Egypt) I have called Middle Helladic III, because it is clearly only a continua-

¹ *B. S. A.*, XXII, p. 186; Wace, in *J. H. S.*, 1921, p. 262. Mr. Wace dates the Shaft-Graves from the end of MH II down to MH IV (his LH II). It is to be noted that the gold masks from these graves have the Hellenic profile; Lehmann-Haupt, in Gercke-Norden, III, pp. 4-5.

² V. sec. VI, The Kretan Problem, p. 28, *infra*.

³ Nilsson thinks there was such a raid in LM I (v. his review of Beloch, *Gr. Geschichte*, I, in *Gött. Gel. Anz.*, 1914, pp. 526 sq.). The palace at Knossos was "wholly remodelled at the end of the Middle Minoan period, and apparently largely altered and enlarged in the Late Minoan period" (Hall, p. 46).

tion, or step in advance, of the previous period. The Minoan influence, which began to enter the Helladic mainland in Middle Helladic II, gradually increased and became stronger in the two following periods. The transition from Middle Helladic II to the succeeding period was a gradual one. In fact, the Sixth Shaft-Grave at Mykenai overlaps the transition between these two periods.

This period, Middle Helladic III (1600–1500 B.C.), Wace and Blegen have designated as Late Helladic I, not because they recognize the occurrence of a cultural ‘break,’ or the entry of a new race or people, but because they have based their system of classification on the Minoan system of Sir Arthur Evans. In making Late Helladic I contemporary with Late Minoan, they have allowed stylistic reasons to determine the dividing point between Middle and Late Helladic, although admitting that the transition was gradual.

But the date that should be taken as the dividing point between Middle and Late Helladic is the date that marks the end of the régime or civilization of the Middle Helladic people, namely, *circa* 1400 B.C., the approximate time when a new wave of invaders from the North (the “Achaïans”) appears to have brought an end to the régime of the “Minyans,” or what I have called the “Minyan Age” (*circa* 2000–1400 B.C.). Hence, the two periods which have previously been called Late Helladic I and II, I have designated as Middle Helladic III and IV.

(3) Middle Helladic III (*circa* 1600 to 1500 B.C.)¹

This period is characterized by the rapid increase of Minoan influence. The characteristic pottery of this, as of the following period, is the so-called Mykenaïan pottery with lustrous paint.² The pottery is still Helladic, but it is treated with Minoan technique. The monochrome ware is Yellow Minyan, covered with red or black lustrous paint; and many of the Helladic, or Minyan, shapes persist. The pottery is wheel-made, of fine fabric, with the decoration in lustrous paint. The patterns are usually linear, and the most frequent is the

¹ This period is contemporary with the early reigns of the XVIIIth Dynasty in Egypt; Hall, pp. 36–37, and table on p. 228.

² The pottery of this and the following period is described in *Korakou*, pp. 36–59 (there under LH I and II).

spiral. Matt-painted ware is limited in use to coarse ware (Group I), while Gray Minyan is very rare. The Sixth Shaft-Grave at Mykenai received its second interment, and other shaft-graves were made and used in this period.¹ The numerous gold ornaments and objects of gold and of other materials found in these shaft-graves attest the wealth of the "Minyans" at Mykenai. The first palaces at Mykenai and Tiryns may have been constructed in this period (if not in the next period).² It is thought that the influence of Krete is to be found in their architecture and decoration.

(4) Middle Helladic IV (*circa* 1500 to 1400 B.C.)³

The previous period, from the point of view of wealth, might be said to be the Golden Age of Mykenai, "rich in gold"; but this Fourth Middle Helladic Period (the division between the two periods is purely a stylistic one) marks the high point in the progressive development, along artistic lines at least, which had been going on for centuries. In Middle Helladic IV the influence of the Minoan culture reached its height, and we may see here the zenith of the "Minyans'" régime. All the evidence confirms the belief that this was the Golden Age, or at least the continuation of the Golden Age, of "Minyan" culture, as it was of the Minoan at Knossos.

That this prosperity was not confined to Mykenai is shown by the results of excavations in other parts of the Peloponnesos.⁴ The vases and objects found in the tombs at Kakovatos (the Triphylian Pylos), at Messenian Pylos, at Vaphio, and at the various sites in the Argolid and the Korinthia, indicate that this high state of culture was quite uniform throughout the Peloponnesos, even if the other localities did

¹ I am indebted to Mr. Wace for this information; and *v. n.* 1, p. 24.

² Mr. Wace writes (Mykenai, June 4, 1922), "I am inclined to think the earlier palace at Mycenæ is of LH I, but there is no definite evidence, but rather an accumulation of hints which point in this direction." Fimmen dates it in this period also (p. 143). The palace at Tiryns is dated in this or the following period, in LM I by Fimmen (p. 214), in LM I-II by Rodenwaldt, *Tiryns*, II, pp. 5, 65 and 201 and *Ath. Mitt.*, 1911, p. 248 (wall-painting in LM II); but either date is in my Middle Helladic Period.

³ This period falls between the reigns of Thothmes III and Akhenaten, i.e., roughly, the fifteenth century; Hall, p. 37.

⁴ Kakovatos, *Ath. Mitt.*, 1908, pp. 295 sq.; Vaphio, *J. H. S.*, 1904, p. 317; *v.* Fimmen, pp. 9-13, for references to various places.

not attain the wealth and consequent power of Mykenai, the centre of this mainland culture.

This period, Middle Helladic IV, marks the highest development of ceramic art. Here we reach the culmination of a process that had been going on since the Second Middle Helladic Period — the process of the fusion of "Minyan" with Minoan art.¹ Minyan shapes still continue, but the Minoan influence in the technique is still stronger. The decoration ceases to be merely geometric; naturalistic designs, consisting of floral motives and nautical plants and animals, form the decoration of vases of graceful shapes. In "Ephyraean" ware,² which is practically Yellow Minyan with a painted decoration, we have the Helladic or mainland shape and technique with the Minoan decoration. Gray Minyan occurs so rarely in this period that it seems no longer to have been manufactured. Mattpainted ware (Group I) still continues to be used for coarse vessels, but to a less degree than in the previous period.

Even in the painted vases which show strong Minoan influence in the shape as well as in the decoration, the fabric is different from that of the Kretan vessels,³ so that these vases are of local mainland manufacture, perhaps the work of Kretan potters who may have been brought over to the mainland. Or the wealth of the "Minyan" chieftains may have induced a band or bands of potters to come over to Hellas. To-day the potters of Thrapsano in Krete travel about the island, settle down near a bed of clay, and make pots which they sell to the neighboring villagers, and then move on to another locality. And the potters of Siphnos journey to the other islands of the Kyklades, to make and sell their pottery.⁴ It is quite possible that this same custom existed in Minoan or Helladic times, when political conditions permitted.

The Fourth Middle Helladic Period brings to a close the era of the "Minyan" culture, or the régime of the "Minyans" in the Peloponnesos. The civilization of the "Minyans" was more truly "Hel-

¹ Cf. *Korakou*, p. 44.

² "Ephyraean" ware, *Korakou*, p. 54, Plates VI and VII; *B. S. A.*, XXII (1916-1918), p. 182, Pl. X.

³ The squat-bowl or alabastron, for example; *Korakou*, p. 50. Cf. n. 2, p. 31, *infra*.

⁴ I owe this information to Mr. Wace or Dr. Blegen.

lenic" or Helladic than any other that appeared in the Peloponnesos; in time, however, as we have seen, the influence of the higher culture of Krete affected it materially, and it became to a large degree Minoan. There seems to have been a distinct 'break' between Middle Helladic IV and the Late Helladic Period. This is indicated not only by the deterioration in the pottery, but also by the appearance of new shapes and by technical changes in the vases of the old types. The 'break' is accompanied by so many phenomena that are indicative of a change in régime, that the coming of a new people into the Peloponnesos is postulated.

Before I bring forward the evidence for this invasion or migration, it seems best to discuss the question of a Kretan conquest of the Peloponnesos.

VI. THE KRETAN PROBLEM

As there are still some who believe in a Kretan colonization or conquest of the Peloponnesos or parts thereof,¹ it seems worth while to present a few arguments against this theory. It need hardly be said that in early times the acquisition by "peaceful penetration," of a district already settled, is not to be thought of. Where there was a Minoan colony, there must have been a Minoan conquest. And if there was a conquest by these foreigners, there should be some evidence of this event. But the only evidence of a Kretan conquest is the Minoan influence apparent in the arts and architecture of the people of the mainland, and this is easily explicable. For a region to be influenced materially by the higher culture of a neighboring land is a natural phenomenon.

There are several proofs of a negative character which clearly refute the theory of a Kretan conquest.

(1) The Minoan influence filtered into the Peloponnesos gradually. It is first noted in the so-called "D" wares and the Polychrome Matt-painted ware (Group III), which begin to appear in Middle Helladic II, and in small quantities.² If there had been a conquest, there would

¹ Hall, p. 61, "In the Peloponnese the Minoans must have established themselves during the Middle Minoan age." Meyer (I (1921), 2, sec. 520, p. 795) and Beloch (I, 1, p. 120) oppose this view.

² *Korakou*, pp. 32 sq.

have been the sudden appearance of Minoan wares, and in considerable quantities.

(2) Although the mainland palaces came to be rather Minoan in style and construction, yet the characteristic feature of the Helladic palace was the Northern megaron with its portico, the apparent prototype of the Hellenic temple *in antis*. If there had been a conquest, the Minoans would have built their houses or palaces in accordance with their own native Kretan style and plan.¹ Then too, may it not be significant that in the frescoes found in the palace at Tiryns the arrangement of the hair of the woman carrying the casket "has no analogies in Cretan art," and that in the large hunting scene the costumes are characteristic of the Helladic mainland and are different from those of Krete?²

(3) Again, if Minoans had settled on the mainland, they would have left some trace of the Minoan writing. But except for a few signs scratched on one or two vases,³ there is no trace of the Minoan script on the mainland.

(4) A conquest would be evidenced by some 'break' in the cultural sequence, but none can be found. The first contact with Krete appears in Middle Helladic II with the *gradual* infiltration of 'Minoanized' (not Minoan) wares, and there was certainly no conquest at this time (*v. Section I, supra*), or in the following period. The division made *circa* 1600 B.C. between Middle and Late, in both the Minoan and Helladic systems, is misleading. The First Late Minoan or Late Helladic Period (my Middle Helladic III) merely marks a step in advance of the previous period. The increased evidence of Minoan influence in Middle Helladic III is only the continuation of the process of infiltration

¹ Point brought out by Beloch (I, 1, p. 121). Much has been made of the similarity between the Knossian and the Late Helladic palace at Mykenai; but the ruinous state of the Mykenaian palace remains necessitates restorations; yet withal, the megaron is distinctly non-Kretan. A comparison should be made with the Tiryns palace, which is better preserved. It is certainly not thoroughly Kretan.

² The characterization of these frescoes I have taken almost *verbatim* from Miss Gisela M. A. Richter's *Handbook of the Classical Collection* (of the Metropolitan Museum of Art, New York City), p. 34 (4th edition, 1922). To resort to the meticulous, I might add that there is a representation of a lady with "auburn hair" on a piece of fresco from the megaron at Mykenai; *v. J. H. S.*, 1921, p. 263. Is not "auburn hair" an "Achaian" or Helladic trait, rather than a Kretan one?

³ Sir Arthur Evans, *Scripta Minoa*, pp. 54 sq., figs. 31 and 34.

that had begun in the previous period, and continued to increase till it reached its height in Middle Helladic IV. Furthermore, the shaft-graves at Mykenai form an unbroken series from the middle of Middle Helladic II down to (or possibly into) Middle Helladic IV, and clearly indicate a continuity of culture in this region. So there does not seem to be any evidence of a change of régime during the Middle Helladic Period.¹

The first real 'break' that occurred after a contact was established between the mainland and Krete is that which is the mark of division between Middle Helladic IV and the Late Helladic Period. But a Kretan conquest at this date (*circa* 1400 B.C.) is out of the question. As I have said before, the invasion which brought about the destruction of Knossos at this time was from the North, and the Kretans would have been driven in every direction except toward the Helladic mainland.

So, it seems to me, the theory of a Kretan or Minoan conquest is rendered untenable both by reason of the lack of positive evidence, and because of the abundance of negative evidence that may be adduced.

VII. THE "ACHAÏAN" INVASION

1. *Archaeological Evidence*

There is abundant archaeological evidence for an invasion of the Peloponnesos at approximately the end of the fifteenth century B.C., that is, at the end of Middle Helladic IV (LH II). A study of the pottery alone is sufficient to convince one that a break occurred between the Middle and Late Helladic Periods, and the break is of such a character that it can be reasonably explained only by an invasion and conquest by a new people. It is true that the evidence at Korakou does merely show a change of level of habitation and gives no indication of any cataclysm or sudden disaster. However, the finds at Korakou do suggest a 'break'; and the excavations at other sites

¹ The theory of a Kretan conquest receives another death-blow. Much has been made of the great quantity of Kretan imported vases found on the mainland. But recent studies of Helladic pottery have led Wace and Blegen to conclude that a Kretan import is comparatively rare in Hellas, and that a majority of the vases hitherto considered to be imports are not Kretan, but of local manufacture; *v. n.* 2, p. 31, *infra*.

(notably at Mykenai) point conclusively to the establishment of a new régime in the Peloponnesos at this time.

(1) *Pottery*. A comparison of the pottery of Middle Helladic IV with that of the Late Helladic Period illustrates clearly the change that took place along artistic lines between these two periods. The pottery of the Late Helladic Period (LH III, *circa* 1400–1100 B.C.) differs from that of the preceding period in the following ways:

(a) Deterioration in decoration and in technique characterizes the pottery of this period. To quote from Dr. Blegen's *Korakou*: "Shortly after its [the potter's art] highest stage of perfection was attained, a gradual deterioration set in, affecting both fabric and decoration; and, degenerating steadily, Mycenaean pottery finally terminated in the poorly made vases with lifeless, conventionalized designs of the end of the Third Late Helladic (Late Minoan III) Period."¹ Then too, there was a deterioration in the technique of the unpainted vases of this period. The clay is inferior in fabric and in baking to that used in the previous period, and the clay slip is often powdery and not so fine or so smoothly polished as formerly. Clearly artistic feeling was on the decline.

(b) In the Late Helladic Period the interior of the vase is almost always painted, usually brownish black in color. This Kretan characteristic rarely occurs on the mainland in the previous periods.²

(c) Late Helladic (LH III) pottery seems to be much further removed from originals in metal than the vessels of the previous period. The splaying flat rim with metallic profile which was characteristic of vases of Middle Helladic III and IV has disappeared, and the former wide flat ribbon handle has for the most part been supplanted by a small loop handle.³

(d) The greatest change from the previous period is to be seen in the characteristic shapes of the Late Helladic Period. The stemmed goblets, "cut-away-neck" jugs, and the "Vaphio-shaped" cups no

¹ *Korakou*, p. 44.

² A criterion, recently observed by Wace and Blegen, is that the interior of a Kretan vase is usually painted, while not until the Late Helladic Period (their LH III) was it customary to paint the inside of a vase on the mainland. Messrs. Wace and Blegen have kindly allowed me to make use of this observation of theirs, which will appear in an article by them on the general problem of Late Helladic pottery.

³ *Korakou*, p. 62.

longer occur. New shapes appear: (1) the high-stemmed kylix, (2) the typical Late Helladic (LH III) bowl or krater, and (3) the *Bügelkanne* or stirrup vase.¹

This sudden appearance of new shapes, accompanied by the other changes or differences in the pottery of the Late Helladic Period, points strongly to a sharp 'break' in the civilization on the mainland at the end of Middle Helladic IV (*circa* 1400 B.C.). The ceramic evidence is supplemented by other evidence indicative of a still more radical change; so that I believe that an invasion of a new people was the cause of this 'break.'

(2) A change of level of habitation between Middle Helladic IV and the Late Helladic Period was found at Korakou, and excavations at other sites show that the Late Helladic (LH III) stratum was distinct from that of the former period.

(3) The fibula (περόνη) makes its first appearance in this period, and it came from the North.² Although not every change or innovation in costume is to be explained by an invasion, nevertheless, in the light of our other evidence, it would seem that the fibula was brought down from the North by a migration.

(4) A still more striking innovation in this period is the "Mykenaian" clay figurine which is so characteristic of the Late Helladic Period (LH III).³ These figurines, usually primitive representations of a female deity with or without a child at her breast, are found in comparatively large numbers at all "Mykenaian" or Late Helladic sites. They are too well known to require a description, but it may be worth while to call attention to the raised round band which runs up the back of the figurine and ends on the top of the head. This is usually taken as the braid of hair, but it has been suggested to me that it represents a snake and is an attribute of the deity.⁴

¹ *Korakou*, p. 62. The *Bügelkanne*, or stirrup vase, appeared before this period in Krete, but the type became popular in this Late Helladic Period, and represents merely a change in artistic taste.

² Studniczka, *Ath. Mitt.*, 1887, pp. 8-24; Ridgeway, *The Early Age of Greece*, I, pp. 560 sq.

³ Furtwängler, *Ägina; das Heiligtum der Aphaia*, pp. 471 sq.; *Korakou*, pp. 106 sq.

⁴ Suggested by Dr. L. B. Holland, Architect of the American School at Athens, 1920-1922.

(5) The Late Helladic Period ushered in a new architectural era. Palaces were built at Mykenai and Tiryns, and quite probably at other sites.¹ The Northern megaron still remained the characteristic element of the palace, but the complex house-plan, the flat roof, and the decoration were Minoan in character.

(6) Another conspicuous feature of the Late Helladic Period is the so-called "Bee-hive" or Tholos tomb. Although tombs of this type have been found in the Peloponnesos that date in the previous period, or even at the end of Middle Helladic III,² still the great age of the "Bee-hive" tomb is the Late Helladic Period. The majority of these tombs and the largest ones are of this age. The famous tombs of "Atreus" and of "Klytaimnestra" are dated by Mr. Wace in this period.³

The "Bee-hive" tomb probably developed from the rock-cut chamber tomb with dromos. It is significant that this type of tomb does not really appear in Krete until Late Minoan III (Late Helladic Period), and the majority of these Kretan tombs are to be dated in the transition period between Late Minoan III and the Geometric Age.⁴ Therefore, this type of tomb may have been introduced into Krete by the Helladic or mainland invaders of the island.

Although the "Achaians" did not introduce the "Bee-hive" tomb into the Peloponnesos, they at least popularized the type, and certainly constructed the largest and most magnificent ones. Therefore, these tombs must be reckoned along with the real innovations as evidence of a new régime in the Peloponnesos. It seems that Mykenai and undoubtedly the rest of the region received new rulers in the Late Helladic Period (LH III). The new people apparently wished to house

¹ Wace, *J. H. S.*, 1921, p. 263: "the existing palace, which seems to date back to the beginning of the Third Late Helladic Age." For Tiryns, Rodenwaldt, *Ath. Mitt.*, 1911, pp. 198 sq. and 221 sq. Cf. p. 26, n. 2, *supra*.

² According to Mr. Wace, the tholos tombs fall into three groups: (1) those which began at the end of MH III (his LH I); (2) those of MH IV; and (3) the group which includes the so-called "Treasury of Atreus" and the "Tomb of Klytaimnestra" of the beginning of the Late Helladic Period. He adds that "these views on beehive tombs may have to be modified by the results of investigations which we are now carrying on" (Letter of June 4, 1922).

³ *J. H. S.*, 1921, pp. 264-265; cf. preceding note.

⁴ Fimmen, p. 61.

their deceased rulers in more elaborate tombs than those in which the previous "Minyan" chieftains had been buried.

(7) *The "Cyclopean" Walls.* One of the most conspicuous features of the Late Helladic Period, one that clearly shows a change in régime, is the construction of the great circuit walls that are to be seen at Mykenai and Tiryns. As a result of his recent investigations at Mykenai, Mr. Wace has come to the conclusion that the great akropolis circuit wall, with the famous "Lions' Gate," is to be dated in the Late Helladic Period (LH III).¹ The great "Cyclopean" walls of Tiryns are to be dated in this same period, and doubtless also the walls of this type that are found at other sites.² Such fortification walls suggest that the builders were a warlike people who had learned from experience the advantage or value of a great circuit wall.

To summarize the archaeological evidence of a 'break' *circa* 1400 B.C., I shall merely repeat the characteristics and innovations of the Late Helladic period. They are:

(1) Disappearance of old, and the appearance of new pottery shapes, and the deterioration in technique and decoration of Late Helladic pottery.

(2) Change of level of habitation. Unimportant by itself, but a necessary concomitant phenomenon to a 'break.'

(3) The Fibula.

(4) The "Mykenaian" figurines.

(5) The new palaces at Mykenai and Tiryns, etc.

(6) The large "Bee-hive" tombs.

(7) The so-called "Cyclopean" akropolis circuit walls.

Although any one of these points, when taken into consideration alone, may not be sufficient evidence for the establishment of a new régime in the Peloponnesos, yet when we consider them together, it seems almost obvious that a people (from the North, of course) invaded and conquered the Peloponnesos at the end of the Middle Helladic period, that is, *circa* 1400 B.C.

¹ *J. H. S.*, 1921, pp. 262 and 265.

² Fimmen, p. 32. In this connection should be mentioned the Mykenaian "military roads," which also form a conspicuous feature of the Late Helladic Period. The remains of these roads are still visible to-day in the Argolid and in the valley of Kleonai; v. Steffen, *Karten von Mykenai*.

2. *Traditions*

Although tradition does not afford any definite evidence for an invasion of the Peloponnesos at *circa* 1400 B.C., the time indicated by archaeology, yet a hint of an invasion prior to the traditional "Dorian invasion" may be contained in several legends. The very fact that there was a tradition about the *Return* of the Herakleidai suggests an earlier migration of this people or of kindred tribes into the Peloponnesos. Herodotos' account of the Dryopians who settled in Asine and Hermione implies that it was generally believed that this people had come from Central Hellas to the Argolid before the "Dorian Invasion."¹ Then, too, there is the tradition that the Herakleidai had attempted to invade the Peloponnesos about a century before their traditional entry.² It is not at all impossible that the "Achaian" Invasion of *circa* 1400 B.C. (at least the first wave of it appears to have arrived in the Peloponnesos about this time) is hinted at in these traditions; that is, we have here a lingering trace, or a survival, of the memory of the event in these legends.

Tradition at least supplies a name for the people who occupied a part of the Peloponnesos in the Late Helladic Period. According to "Homer," these people were the Achaioi, and an Achaian chieftain ruled in "Mykenai, rich in gold." But it is quite clear that the original home of the Achaioi was Phthiotis, or, taking the term in a broader sense, in Southern Thessaly and the Spercheios River valley. This is clear from the Epos, which localizes the Achaioi and their kinsmen there more often and more definitely than in the Peloponnesos.³

However, the tradition that locates the Achaioi in the Peloponnesos is so well founded that the name Achaioi can be used of some of the Late Helladic Peloponnesians, not as a mere arbitrary label, but as a name with some historical basis. First, there is the evidence from

¹ Macan on Herodotos, 8, 73. The Dryopians were considered to be non-Dorian, but the tradition about them shows that there was a belief that a migration had occurred into the Peloponnesos (where lived the autochthonous Arkadians, etc.) before the "Dorian Invasion."

² Herodotos, 9, 26; Apollodoros, 2, 8, 2; Paus., 8, 5, 6; Diodoros, 4, 58 (Diodoros gives the interval as 50 years).

³ The Achaioi; v. Paul Cauer, *Grundfragen der Homerkritik* (3d ed., 1921), I, pp. 279 sq.

the Epos, and although I believe in the "transmutation" of Thessalian Argos and various other elements of Thessalian saga,¹ still, I believe also that the bards of the IXth or VIIIth century B.C. had reason to think that there were Achaioi in the Peloponnesos a few centuries before their time, if not during their own age.² Then, of course, the fact that one of the districts of the Peloponnesos was named Achaia presupposes the presence there at some time of a tribe named Achaioi. Therefore, if we find Achaioi at home in Thessaly, but also present and ruling in the Peloponnesos, the conclusion that we must draw in the light of the other evidence is obvious: the Achaioi came down to the Peloponnesos in a migration.

There will be additional proof for the historical existence of Achaioi in the Peloponnesos, and thereby further support for the theory of the "Achaian" Invasion, if evidence can be adduced for the presence in the Peloponnesos of other Thessalian (or "Northwest Dialect") tribes who were akin to, or allied with, the Achaioi. In the Epos, the Achaioi, 'Hellenes,' and Myrmidons are used synonymously,³ so that we must think of these three peoples as kindred tribes, or clans of a common tribe. The Danaoi also appear to have been a Thessalian tribe. As Cauer has pointed out, they are particularly associated with the Myrmidons and Achilleus;⁴ and the epithet of the Danaoi — *ταχύπωλοι* — certainly argues for their Thessalian provenance.⁵ If it is not significant, it is interesting to note that Pindar calls the Danaoi *ξανθόκομοι*.⁶ Several "Achaians," notably Achilleus, were called "blond-haired."⁷

¹ The "transmutation" theory: Cauer, *op. c.*, pp. 284 sq.; Beloch, I, 1, p. 184; Hall, p. 76.

² It is to be noted that the Epos, with all its Peloponnesian infiltrations, does not even hint of a change of people in the Peloponnesos; and in the sixth century Kleomenes, the King of the most "Dorian" of states, apparently felt sure that there were Achaians (certainly one) still in the Peloponnesos (Herodotos, 5, 72, 3).

³ *Iliad*, 2, 683-684, *et al.*; cf. Thuk., 1, 3. I am using 'Hellenes' (in single quotation marks) in the narrow sense of the original tribe in Southern Thessaly.

⁴ Cauer, *op. c.*, p. 282; *Iliad*, 11, 797; 19, 401.

⁵ *Iliad*, 4, 232; 8, 161, etc.; v. Cauer, *op. c.*, pp. 282 and 283, n. 36; Wilamowitz, *Herakles* (2), I (1895), p. 17, n. 34, and GDI 347.

⁶ Pindar, *Nem.*, 9, 17.

⁷ *Iliad*, 1, 197; Beloch, I, 1, p. 93. The fresco of the "lady with the auburn hair" may be of interest in this connection; v. *supra*, n. 2, p. 29.

These four peoples and other North or Central Helladic peoples are also found in the Peloponnesos.

(1) Achaioi. As has been shown, these people were also in the Peloponnesos.¹

(2) Myrmidons. The home of the Myrmidons was around Phthia,² but there is good reason for believing that Myrmidons once settled in the island of Aigina.³

(3) 'Hellenes.' Their home was in 'Hellas' in Southern Thessaly (perhaps earlier around Dodona),⁴ but there is evidence for the presence of 'Hellenes' in Aigina, and probably in Lakonia too.⁵ Over a century ago Karl Otfried Müller suggested that 'Hellenes' had migrated to the Peloponnesos,⁶ and J. B. Bury has explained the disappearance of the 'Hellenes' from Thessaly in "historical" times by a migration.⁷ And there is evidence in support of this theory.

(4) Danaoi. The ταχύπωλοι Danaoi who appear to have been originally a Thessalian tribe are localized also in the Argolid by a strongly-founded tradition.⁸ Although the name was sometimes used loosely for the Hellenes (national name) in general, yet the many traditions about Peloponnesian Danaoi make it appear evident that more than a "poetic transmutation" was responsible for the fact that Aischylos, Pindar, and others located the Danaoi in the Peloponnesos. There were actually Danaoi in the Peloponnesos, it seems to me.

¹ Caer, *op.c.*, pp. 279 sq.; Strabo, 8, 365, 383; Paus., 5, 1, 1.

² *Iliad*, *passim*. I believe in the historical existence of a tribe called Myrmidons, i.e., in Helladic times.

³ *Prehistoric Aigina*, ch. II. A. Gercke (*Die Myrmidonen in Kyrene*, in *Hermes*, 1906, pp. 447 sq.) believes that Myrmidons wandered down through Southern Hellas and eventually to Kyrene. But I believe that he has made use of secondary tradition, i.e., that the Myrmidons were later connected with the Argonaut legends, and so, later brought to Kyrene.

⁴ Aristotle, *Meteor.*, 1, 353a; v. Beloch, I, 1, p. 332.

⁵ Hellenes in Aigina: evidence summed up in my *Prehistoric Aigina*; cf. Pind., *Pæan*, 6, 125, *Nem.*, 5, 10; Theophrastos, *De sign. pluv.*, 1, 24. In Sparta: cf. Plut., *Lyk.*, 6, p. 43.

⁶ *Aegineticorum liber* (Berlin, 1817), IV, sec. 5.

⁷ J. B. Bury, *History of the Name Hellas and Hellenes*, in *J. H. S.*, 1895, pp. 217 sq.

⁸ Pind., *Pyth.*, 9, 112, *et al.*; Aischylos, *Suppliants*; Euripides, frg. 230 (Nauck, *Tb.* 1892); Paus., 2, 19, 3.

(5) Eleians. Although there is no trace of Eleians (or Valeioi) outside of the Peloponnesos, yet it is clear that they also came from the North. They appear to have been just a branch of the Aitolians,¹ who had pushed on and crossed over to the Peloponnesos. The traditions connecting Eleians and Aitolians, the apparent existence of a cult of Oxylos in both districts, and the similarity of the dialects show clearly that the Aitolians and Eleians were closely related.²

The history of Elis gives one a good idea of the manner in which an invading people establishes itself in a new land. The Valeioi, or Eleians, apparently had to fight for a foothold, and not till after many centuries of periodic warfare did they succeed in becoming dominant in the western part of the Peloponnesos. The peoples who resisted the Valeioi were principally the Epeians, Arkadians, Pisatans and Triphylians. In the *Iliad* Nestor tells of the wars waged by the Pylians (whom he calls Achaioi, conveniently enough for this paper) against the Epeians and Arkadians,³ and the later Eleians of the "historical" period were engaged in periodic struggles with the Pisatans and Triphylians.⁴ All these peoples I would classify under the head "Minyans,"⁵ the people that dominated the Peloponnesos in the Middle Helladic Period, and later were driven back into the interior.

Of course, in the Epos the Epeioi are listed among the Achaian forces in the siege of Troy; but there is no reason for believing that a people from the western part of the Peloponnesos (or from any part of the Peloponnesos, for that matter) ever took part in an expedition to the Troad in the Bronze Age. The Epeians are associated with the Arkadians, and both fight against the Pylians.⁶ The Pisatans similarly, by reason of their associations and geographical position, were probably related to the Arkadians. Passages from Strabo imply

¹ Cf. Herodotos, 8, 73, 2 — Elis, a city of the Aitolians.

² Ephoros, in Strabo, 10, 463; Apollod., 2, 8, 3, 4; Strabo, 8, 354, 357, 389; 10, 463; Paus., 5, 1, 3; v. Beloch, I, 2, p. 85.

³ *Iliad*, 7, 133 sq.; 11, 670 sq. (against Epeioi and Arkades).

⁴ Beloch, I, 1, pp. 90, 386.

⁵ Arkadians are actually mentioned. Other tribes, e.g., the Minyans, Kaukones, etc., might be mentioned along with them; v. Herodotos, 4, 148; Strabo, 8, 337, 347, 355, 357.

⁶ *Iliad*, 7, 133 sq.; 11, 670 sq.

this.¹ The Triphylians are clearly closely related to the Arkadians. This is attested by the statements of ancient writers.² In fact, it appears from a remark of Pausanias that Elis was sometimes regarded as a part of Arkadia.³

As for the date of the Aitolian-Eleian invasion, there is only indirect evidence. According to tradition, Oxylos, the reputed *oikistes* of Elis, came over from Aitolia with the returning Herakleidai and settled among the Epeians.⁴ But a closer approximation as to Eleian chronology may, perhaps, be attained from a consideration of the evidence in the *Iliad*. In Nestor's wars with the Epeians and Arkadians we have apparently another instance of the conflict between the "Arkadian-tongued" "Minyans" and the northern invaders. Just as the "Achaian" Invasion, in which Achaioi, 'Hellenes,' Danaoi, etc., took part, brought about the end of the "Minyan Age" and régime of the "Minyans" in the eastern part of the Peloponnesos,⁵ so here in Elis the invading Aitolian-Eleians assailed the "Minyans" and wrested from them part of their territory.

Therefore, I connect the invasion of the Eleians (Valeioi) with the "Achaian" Invasion, and would date it at the end of the Middle Helladic Period. Both attacks upon the civilization of the "Minyans" would be practically synchronous. That Elis was inhabited in the Middle Helladic Period is evidenced by the remains that have been brought to light.⁶ Olympia, Pisa, Triphylian Pylos (Kakovatos), and possibly Arene were inhabited in this period. The Late Helladic Period in Olympia is represented by one solitary sherd at the most.⁷ (It might be dated earlier.) Might a causal relation be established between the "Achaian" Invasion of *circa* 1400 B.C. and the apparent abandonment of this Middle Helladic site? Of course, this absence of Late Helladic remains may be accidental.

¹ Strabo, 8, 355, 357.

² Thuk., 5, 31, 2; Xen., *Hell.*, 7, 1, 26; Polyb., 4, 77, 8; Paus., 10, 9, 5.

³ Paus., 5, 1, 1.

⁴ Ephoros in Strabo, 10, p. 463; Strabo, 8, 389, etc.

⁵ *V. supra*, p. 17.

⁶ Fimmen, pp. 9-10; *Ath. Mitt.*, 1908, pp. 295 sq. (Kakovatos).

⁷ *Ath. Mitt.*, 1911, p. 177, Abb. 20a. This sherd may belong to the Middle Helladic Period, i.e., it may be classed by Wace and Blegen as of LH I or II. If so, the theory that the site was abandoned (or destroyed?) at the end of the MH period is strengthened.

Further evidence for the presence of Achaioi in the Peloponnesos may be given indirectly by an Egyptian document of the XIXth Dynasty which records an attack made upon Egypt *circa* 1230 B.C. by "Northerners coming from all the lands." Among the "Northerners" repulsed by Meneptah were the Akaiwasha, who have been identified with the Achaioi of Hellas.¹ From another Egyptian document (of the XXth Dynasty) we learn that Danuna (or Danauna) took part in the great invasion which was halted in Palestine by Rameses III *circa* 1196 B.C.² The Danuna have been identified with the Danaoi, who we have seen were native to Thessaly and had wandered southwards. Undoubtedly it was the more advanced Peloponnesian Danaoi, and not the *ταχύπωλοι* Danaoi of Thessaly, who took part in this Egyptian expedition. And likewise the Achaioi who had joined the expedition in Meneptah's reign were in all probability from the Peloponnesos. Thessalians did not take any part in the colonization of the South Aegean or of the Mediterranean. But Peloponnesians for centuries had been in contact with Krete, Kypros, and other South Aegean islands, and they knew of Egypt through Kretans, if they had not been in more direct contact with the Nile-land.

The Akaiwasha and Danuna may have come directly from the Peloponnesos, or from lands colonized from the Peloponnesos. We should expect "Achaians" to appear on the seas after a stay of almost two centuries in the Peloponnesos; and, besides, we know that the Late Helladic Period was a period of movements and disturbances.³ Incidentally, this Egyptian evidence shows that the Achaioi and Danaoi were in Southern Hellas, or the South Aegean before the time of the traditional "Dorian Invasion."⁴ So there is some evidence for dating the migration of the Achaioi and related tribes at the end of the Middle Helladic Period, when there is a 'break' indicated by archaeology.⁵

¹ Hall, p. 377, also p. 70; Akaiwa-sha — Ἀχαιοί.

² Hall (pp. 380-381) translates the document.

³ "The isles were restless, disturbed among themselves at one and the same time." (Hall, pp. 69, 380.)

⁴ I.e., before the eleventh century, when there is evidence of an invasion of the Peloponnesos; *v. infra*, sec. VIII, p. 49.

⁵ If they (or the first wave of them) came before the eleventh century, they must

All these people who appear to have been in possession of the greater part of the Peloponnesos in the Late Helladic Period (1400–1100 B.C.), I have labelled “Achaians,” using this name as a collective term for several kindred or allied tribes, such as the Achaioi, Danaoi, ‘Hellenes,’ Myrmidons, Valeioi (Eleians), etc.¹ These “Achaians” whose original homes were in Thessaly or in Northwest Hellas were the people who migrated to the Peloponnesos at the end of the Middle Helladic Period (*circa* 1400 B.C.).

3. *Dialects*

I believe that the dialects spoken by the “Achaians” in the Peloponnesos in the Late Helladic Period belonged to the Northwest Dialect group, and from this group was developed or evolved the dialect that came to be known in “historical” times as the Dorian dialect.

In other words, *the “Achaians” were Dorians*, in the sense that they spoke dialects of the group from which “historic” or classical Doric developed.

Of course, all these “Achaian” tribes did not speak one and the same dialect. A common, uniform dialect is the last goal attained in the development of language.² But, for the sake of convenience and to avoid paraphrase, I shall call this stratum of various but related dialects by the name Doric, with the understanding that this name (it might better be called “proto-Doric”) is a quasi-composite term for the Northwest Dialect group, and not necessarily very similar to the Dorian dialect of classical times.

There were three well-defined strata of dialects in the Peloponnesos:³ first, that of the non-Indo-European “Aigaians” of the Early Helladic Period; second, the “Arkadian” dialect of the “Minyans”;

have come at some ‘break,’ and the only ‘break’ evidenced prior to *circa* 1100 is that of the “Achaian” invasion of *circa* 1400 B.C.

¹ Along with these groups may be mentioned the three well-known Dorian tribes, the Hylleis, Pamphyloi and Dymanes; v. Beloch, I, 2, pp. 97 sq.

² Cf. Meyer, *GA* I (1921), I, sec. 39, p. 79.

³ Kretschmer distinguishes four strata of dialects, but I am more inclined to agree with Beloch *et al.*, and discard the Ionic. Kretschmer, in Gercke-Norden, I, pp. 142 sq.; Beloch, I, 1, p. 77, and 2, pp. 88 sq.; v. also Dialect map in Buck, *Greek Dialects*, Plate V.

and lastly, the Dorian. There does not seem to have been an Ionic stratum in the Peloponnesos. Where traces of Ionic have been found, its presence may be due to two causes: either it is a survival of the "Aigaian" dialect Hellenized (some believe that the Ionians were less purely Hellenic in race than the other Hellenes, and were probably a mixture of Indo-European and non-Indo-European elements);¹ or, more probably, Ionic was introduced into the Peloponnesos in some manner after the coming of the Dorian-speaking tribes.

So it is apparent that the Late Helladic "Achaians" spoke dialects akin either to the "Arkadian" or to the Dorian dialect, unless one wishes to believe that they spoke Ionic or some lost dialect, both of which alternatives are highly improbable, if not impossible. *Prima facie*, it seems more probable that the "Achaians" spoke Doric, the dialect of the last invaders who entered the Peloponnesos comparatively soon after the "Achaians," than that they spoke the dialect of the "Minyans," who had come South about six centuries before them. The evidence leads me to believe that the traditional "Dorian Invasion," which took place toward the close of the Second Millennium B.C., was but another wave of the same migration which began *circa* 1400 B.C., and that the traditional "Dorians" were of the same stock as the "Achaians."² Their dialect was probably similar or akin to that of their predecessors, the "Achaians."

But we can more than conjecture that the "Achaians" spoke the Doric dialect (i.e., "proto-Doric," or a Northwest dialect). Negative and positive evidence may be adduced to show that in all probability the "Achaians" did not speak "Arkadian," but did speak Doric. We have seen that the evidence from archaeology and cults points to a difference in race and culture between the "Minyans" and their successors. And a study of the cult and name of Posoidan also indicates a difference in dialect between the two peoples.³

From the Epos we learn that the patron god of the "Achaians" was Zeus. Achilleus, the chieftain of the "Achaian" Myrmidons, calls upon the Zeus of Dodona as his patron deity.⁴ The Aiakidai who are

¹ Hall, p. 67.

² I follow Beloch in his theory that the "Achaians" spoke what we may call the Dorian dialect, but not in his chronology.

³ *V. supra*, p. 21.

⁴ *Iliad*, 16, 233 sq.

"Achaïans" pray to their father Zeus Hellanios in Aigina, according to Pindar.¹ And it seems probable that Zeus Hellanios derived his epithet from the tribe Hellenes who worshipped him.

But the leading deity of the "Minyans" appears to have been Posoidan, and the Posoidan worshippers represent the dialect-stratum preceding that of the later people who worshipped Zeus.² The legends of Posoidan's (Poseidon's) unsuccessful contests with Zeus for Aigina and with Hera (an "Achaïan" or Dorian deity) for Argos reflect more than a change of people; the late comers spoke a different dialect, and in their dialect (more correctly, dialects) Posoidan became Pohoidan, Potidavon, and other forms, according to the locality. Hence, if the "Achaïans" did not speak "Arkadian," they must have spoken a dialect of the Northwest group which immediately followed the "Arkadian," that is (proto-) Doric.

There will be additional proof or support for the theory that the "Achaïans" spoke the Doric dialect, if it can be shown that the 'Hellenes,' who are "Achaïans," used this dialect. The tribe of the 'Hellenes' rose to such prominence that they gave their name to the race that inhabited the Balkan peninsula and the Aegean shores and islands. But undoubtedly it was the Peloponnesian 'Hellenes,' and not the Thessalian 'Hellenes,' who gave their name to the nation; and from what we know of Helladic (early Hellenic) history it is only reasonable to expect this.³ Civilization or culture developed earlier and rose to a higher level in the Peloponnesos than in the Northern part of Hellas. Besides, the "Achaïan" migration brought about the virtual disappearance of the 'Hellenes' from their native home in the North.⁴ And so it is to the Peloponnesos, the other place where 'Hellenes' are found, that we must look for the growth of the name 'Hellenes' from a tribal to a national significance.⁵

¹ Pind., *Nem.*, 5, 10.

² *V. supra*, p. 21. The objection that Poseidon was on the side of the Achaïans at Troy may be set aside; later Posoidan did become a deity of the Hellenes; and besides, the Peloponnesian "Achaïans" probably never saw Troy.

³ Beloch (I, 1, pp. 331-332) suggests that it was through the Delphian Amphictyony that the name Hellenes came to develop its broader significance, and to designate the whole Hellenic people.

⁴ *V. supra*, p. 37, and notes.

⁵ I think that the early rise of the Peloponnesian commercial states, especially

Now the fact that the 'Hellenes' rose to such prominence that they could give their name to the whole race postulates a continued existence and growth on their part. The 'Hellenes,' as we have seen, are to be classed among the peoples who took possession of the Peloponnesos at the end of the Middle Helladic Period,¹ and so they must necessarily have continued to maintain their independence down into (strictly speaking) "historical" times, and also to have grown in numbers and power. Therefore, the traditional "Dorian Invasion" had little or no permanent effect on them. If the 'Hellenes' were not materially affected by the later wave of invaders, their language could not have been changed to any appreciable degree, and so the 'Hellenes' gave their dialect as well as their name to their countrymen. In other words, the dialect that was spoken in the Argolid, for instance, in the classical period was practically the same as (really, derived from) that which was spoken by the 'Hellenes' or "Achaians" of the Mykenaian Age; that is, the 'Hellenes' and other "Achaians" spoke the "North-west" dialect which we may call 'proto-Doric' or, more loosely, Doric.

A third proof that the "Achaians" spoke the Doric dialect has been demonstrated by Beloch.² The dialect of Lakonia, Argolis, Aigina, Achaia, and other places in the Peloponnesos is so closely akin to that of Phthiotic Achaia³ and the Spercheios valley, that we must believe that the inhabitants of these localities were of the same stock. So the Northern and the Peloponnesian "Achaians" in the Late Helladic Period spoke the same dialect, the Doric.

That Achaian and Dorian were not necessarily mutually exclusive terms is shown by the fact that even in the classical period it was common for Achaian and Dorian to be confused. Even Herodotos, or the

Aigina, had much to do with the development of the name, although the evidence is very scanty, I must admit.

¹ *V. supra*, p. 36.

² Beloch, I, 2, pp. 88 sq.

³ Authorities differ as to what was the original dialect of Phthiotis. Cauer (*op.c.*, pp. 271 sq.) and Kern (*Nord-Griechische Skizzen*, p. 35) believe that it was Aiolic, and that the Northwest Dialect came in with invaders, in a period subsequent to the colonization of Aiolis, Asia Minor. Chadwick (*Heroic Age*, p. 283) does not think that Phthiotis was Aiolic land, nor does Beloch. I think that Phthiotis had become "Doricized" by a people of the Northwest Dialect group (long) before the "Achaian" invasion of the Peloponnesos at the end of the fifteenth century (*v. infra*, p. 59).

priestess of Athena, did not know that the Spartan king, Kleomenes, was an Achaian.¹ Apparently Ionians had come to believe that a man who spoke the Dorian dialect was certainly not an Achaian. But that there were some people who had not been so misled by the logographoi as to think that there was a racial distinction between Achaian and Dorian is shown by a passage in Plato's *Laws*: "The exiles came again [to the Peloponnesos], under a new name, no longer Achaians, but Dorians. . . ." ² The "Achaians" spoke a dialect that came to be called Dorian. A native of New Jersey speaks English, but he is an American. The names descriptive of the nationality and dialect of a people are not always identical.

Of course, the "Achaians" spoke various dialects of the Northwest group, and from these developed the various local dialects, with characteristic differences. The Eleian dialect was probably brought into the western part of the Peloponnesos in the same migration that brought in the Dorian of the Argolid. The Dorian and the Eleian dialects both belong to the Northwest group, and quite possibly they were more similar at the time of the invasion than in classical times. The Thessalian place-names found in Elis ³ (Olympia, Peneios, Ephyra, etc.) might have been brought down by the Eleians or some accompanying Thessalian tribe in this "Achaian" invasion, although it is possible that the "Minyans" introduced these northern names centuries previously.

It is not necessary to suppose that only "Achaians" and closely related tribes invaded the Peloponnesos at this time (*circa* 1400 B.C.); probably several Hellenic tribes took part in the great movement of Indo-European peoples which occurred in the fifteenth century B.C., due doubtless to the press of other tribes from the North. The

¹ Herodotos, 5, 72, 3. Kleomenes, the Spartan king who had seized the Akropolis at Athens in 508 B.C., was refused admittance to the hieron of the goddess because, as the priestess said, it was unlawful for Dorians to enter. He replied that he was not a Dorian, but an Achaian.

² Plato, *Laws*, 3, 682E.

³ L. Weniger (*Olympische Studien*, in *Archiv. f. Religionswiss.*, 1920, pp. 41-78) thinks the name Olympia is explained through a Thessalian invasion. I have not been able to see this article, but I should connect the place-names in Elis with the "Minyans," some of whom may have come from Thessaly, rather than with the later "Achaian" Eleians, who may have come down from Epeiros.

Phrygian invasion of Asia Minor¹ seems to have been a part of this same *Völkerverschiebung*, while the west wing of the movement was formed by tribes of the Northwest Dialect group.

We have seen that a people from North Hellas, whom we may with some justification call "Achaïans," invaded the Peloponnesos at the end of the Middle Helladic Period; and further, there seems to me to be sufficient evidence for the hypothesis that the "Achaïans" spoke "proto-Doric," a Northwest dialect, out of which developed the "historic" Doric. It now remains to determine, if possible, how these Dorian-tongued "Achaïans" entered the Peloponnesos, whether in great migratory bands or in small but well-armed divisions, by land or by sea. Some have compared the "Achaïans" to the Normans. According to their theory, the invasion was a conquest by a few "Achaïan" chieftains and their well-armed retainers, rather than a migration of a people; the "Achaïan" chieftain lived in his well-fortified palace, and kept control over his subject people by means of his ever-ready trained armed force.

But it is questionable whether the institution of a "standing army" or a mercenary force existed at so early a period in Hellas. Besides, when we take into consideration not only the Argolid but the whole Peloponnesos, and connect the Eleians with the "Achaïan" invasion, it seems probable that we have to deal with a horde of migrating people, rather than with a few chieftains and their armed forces. Then too, if the 'Hellenes,' apparently one of the leading tribes that made up the "Achaïan" invasion, were able to survive the later "post-Mykenaïan" invasion² and grow to such importance and fame that they gave their name to the Hellenic race, they must have come in great numbers. The fact that the "Achaïans" were able to overcome or drive out the "Minyans" who had been so long in the land, and that they were able to take part in expeditions against Egypt two centuries later, argues for the coming of these "Achaïans" in great numbers. Besides, it has been supposed by some that this migration lasted several centuries.³

¹ Cavaignac, *Hist. de l'Ant.*, I, 1, p. 34; Thallon, *J. H. S.*, 1919, p. 185.

² I.e., the traditional "Dorian Invasion," that which marks the end of the Bronze Age.

³ Nilsson, *Gött. Gel. Anz.*, 1914, pp. 526 sq.: (Achäische Bewegung) "Es dauerte auch über zwei Jahrhunderte, bis die achäische Wanderung ausebbte."

A glance at the map helps solve the problem of the way by which the "Achaïans" came to the Peloponnesos. The western, northern, and eastern coasts were first "Doricized," and later the southern part was taken by the Dorian-tongued "Achaïans." Hence it appears that a great horde of people from the North came down through Central Hellas and crossed the Korinthian Gulf at its narrowest part (or parts), and possibly also entered the Peloponnesos by way of the Isthmos.¹ Some of the invaders went down along the east, and others along the west coast of the Peloponnesos. The "Minyans" who had been driven back into the interior were able to prevent further encroachments into Arkadia on the part of the invaders.

This theory is confirmed by the tradition concerning the "Return of the Herakleidai." There are various forms of this legend,² but in general they agree that there were two invasions attempted about a century apart,³ and that these people came over by way of the Isthmos of Korinth and by boat from Naupaktos, across the narrow strait of the Korinthian Gulf, to Rhion.⁴ In some versions the first attempt at the Isthmos was unsuccessful; but this halt could have been only temporary. When the others had crossed to the west, and had come along Achaia to the Korinthia,⁵ all opposition at the Isthmos would of necessity be brought to an end.

In the tradition of the two invasions I would see the survival of the memory of at least two waves of migrators, though not necessarily just three generations apart. And this agrees with the opinion that

¹ Beloch does not believe that they came by way of the Isthmos, since Boiotia was not Doricized (I, 1, p. 89).

² Herodotos, 9, 26, 2 sq.; Apollodoros, 2, 8, 2; Diodoros, 4, 58; Strabo, 8, 333, 365, 383; Paus., 8, 5, 1 sq.

³ The Tegeans said 100 years (Herodotos, *ibid.*); Pausanias and Apollodoros, three generations; Diodoros, 50 years.

⁴ Apollodoros mentions Naupaktos; Paus., 7, 5, 6: "This time they came, not by the Isthmos of Korinth, as they had done three generations before, but in ships to Rhion."

⁵ The "Arkadians" (my "Minyans") spoke a form of Aiolic, and the "Achaïans" spoke Doric. Perhaps the conflict between Aiolic "Minyans" and the invading Dorian "Achaïans" may be reflected in the tradition that the Dorians had encamped on the Solygeian hill when they made war on the Aiolic Korinthians (v. Thuk., 4, 42, 2; cf. Paus., 2, 4, 3 — Dorians under Aletes, one of the Herakleidai, take Korinth).

the Late Helladic Period was one of movements. The first wave of the invasion, and the most important, as it caused the 'break' along ethnic and cultural lines, I should date at *circa* 1400 B.C., when a 'break' is indicated by the archaeological evidence. Hence, I believe that the tradition of the "Return of the Herakleidai" must be linked with the "Achaian" invasion.¹

Of course, the myth of the "Return of the Herakleidai" was an invention, as Beloch has pointed out,² to account for the presence of Dorians in a land where Homer had localized Achaians. However, I believe that the myth was fastened upon an historical event, namely, a migration, the first wave of which must be placed at the time indicated by archaeology. Any objection that may be made against the application of this tradition to the "Achaian" invasion, on the ground that Herakleidai came along with Dorians, may easily be disposed of. "Achaians" are Dorians, and besides, as the claim of Kleomenes, the Spartan king, shows, the Herakleidai were "Achaians."³

So the "Achaians" seem to have come in great numbers and by land from the North. Those who came down through Akarnania and Aitolia and through Phokis probably crossed over from Naupaktos and the vicinity, where the Gulf narrows to the width of about two miles. Others may have wandered down through Boiotia and entered the Peloponnesos by way of the Isthmos, ultimately at least. Since the "Achaians" seemingly spoke the Dorian ('proto-Doric') dialect, this "Achaian" migration might well be called the Dorian Invasion. However, since we have to deal with another invasion of the Peloponnesos to which tradition has given the name the "Dorian Invasion," I shall reserve this appellation for the event that gave another blow to the Mykenaian culture, which had already begun to decline under the "Achaian" régime.

¹ No objection should be made to connecting legends concerning the Herakleidai with the "Achaians." It was a recognized fact in antiquity that the Spartan kings were Herakleidai, and of Achaian origin.

² Beloch, I, 2, pp. 76 sq., esp. p. 82.

³ Herodotos, 5, 72, 3.

VIII. THE "DORIAN INVASION"¹

According to tradition, the Dorians, a more or less uncultured people from the North, overran the Peloponnesos, destroying and devastating as they went along. The traditional date, as given by Eratosthenes, is 1104 B.C.; and, peculiarly enough, archaeology shows that the Peloponnesos suffered a hostile invasion about the beginning of the eleventh century. It is more probable that the real Dorian invasion began with the "Achaio-Dorian" migration which occurred at the end of the Middle Helladic Period, *circa* 1400 B.C. But it was not of a destructive character, nor did it bring about a "Dark Age." The "Achaians" appear to have been more constructive than destructive. They were inferior to their predecessors only in an artistic sense; the decline that set in as a result of their invasion was purely along artistic lines.

It has been doubted by some² that there was an invasion at the end of the Mykenaian Age, or the Late Helladic Period; but the archaeological evidence cannot be set aside as easily as the traditions. The evidence for an invasion at the end of the Late Helladic Period is most convincing.

(1) The destruction of the palaces at Mykenai and Tiryns, and probably elsewhere,³ appears to have taken place about the same time—at the end of the Late Helladic Period.

(2) Several Peloponnesian sites were destroyed about this time and were never inhabited again. Among the most striking examples are Korakou and Zygouries, and other sites⁴ might be enumerated which ceased to be inhabited after Mykenaian or Helladic times.

¹ Beloch, *Die dorische Wanderung*, in *Rhein. Mus.*, 1890, pp. 555 sq., and *Griechische Geschichte*, I, 2, pp. 76 sq.; Casson, *The Dorian Invasion Reviewed*, in *Antiquaries Journal*, 1921, pp. 199 sq.; Hall, pp. 73 sq.; Lehmann-Haupt, in *Gercke-Norden*, III, p. 8; Nilsson, *Gött. Gel. Anz.*, 1914, pp. 526 sq. (a "Kritik" of Beloch). I have not been able to see Neubert, *Die dorische Wanderung in ihren europäischen Zusammenhängen*, Stuttgart, 1920.

² Principally Beloch, *l.c.*, and Kahrstedt, in *Neue Jhrb.*, 1919, pp. 71-75.

³ *J. H. S.*, 1921, p. 263; Tiryns, II, p. 221; *Ath. Mitt.*, 1911, pp. 198 sq. The "Pottery Shop" at Zygouries was destroyed by fire. A Mykenaian palace may yet be discovered on this site; the Mykenaian (LH) town lay around the foot of the mound.

⁴ In the Korinthia, *A. J. A.*, 1920, pp. 1 sq.; Lakonia, *B. S. A.*, XVI, pp. 4-11.

(3) The introduction of iron which occurred at this time was probably due to the coming of these invaders from the North.

(4) The practice of cremation seems to have begun in the Peloponnesos about the same time as the use of iron.

(5) Although it is disputed by many scholars, some believe that Geometric pottery also was introduced by these last invaders.¹

From this evidence it seems clear that a hostile invasion by a war-like people from the North took place in the Peloponnesos at the end of the Bronze Age. But I believe that it was more of a destructive raid, and that, after these raiders had swept on, the Peloponnesians resumed their former life and ways. Mykenai was rebuilt, for we find it inhabited in the Geometric Age, and likewise Tiryns. Other sites were rebuilt, or settlements arose near old sites, built by the old inhabitants or by the newcomers. The 'Hellenes' apparently continued to grow in importance. The language which I believe was brought in by the "Achaïans" lived on, and came to be known as the Dorian dialect after the name Dorian had come into use for one of the Hellenic ethnic groups. The last invasion was probably the last wave of the migration of peoples of the Northwest Dialect group, and the newcomers were, therefore, "Dorians." Their dialect was probably closely related to the Doric dialect of the "Achaïans."

This last invasion may have been the result of the Thesprotian invasion² of Thessaly and North Hellas in the thirteenth or twelfth century which drove more peoples southward. It is to be noted that Boiotian Orchomenos and Thebes suffered the same fate as the Peloponnesian towns and at approximately the same time. Perhaps the raids on the coastal towns of Krete, which forced the inhabitants to build their homes back in the mountains,³ may have some connection

¹ B. Schweitzer (*Untersuchungen zur Chronologie und Geschichte der geometrischen Stile in Griechenland*, in *Ath. Mitt.*, 1918, pp. 20 sq.) believes that "Geometric" pottery is derived from the non-Minoan pottery of the mainland. So Beloch (I, 1, pp. 220 sq.), who does not believe that it was introduced by the Dorians; cf. Dörpfeld (*Ath. Mitt.*, 1906, p. 206) and Hoernes (*Urgesch. d. bild. Kunst in Eur.*, p. 363); but in favor of the theory are Hall (*Antiq. J.*, 1921, p. 219; cf. his *AEHE*, pp. 62, 74), Leaf, *Homer and History*, p. 330, Nilsson (*l.c.*), and Casson (*Antiq. J.*, 1921, pp. 199 sq.).

² Hall, pp. 73 sq.

³ These towns were destroyed about the middle of the twelfth century. The

with this last invasion of the Peloponnesos. The Kretan coast towns may have been destroyed or raided by "Achaïans" who had been driven out of the Peloponnesos by the "Dorians," but the destructive character of the raid on Krete might argue for its being the work of the "Dorians." And yet we have seen that "Achaïans" were not above taking part in hostile or marauding expeditions; witness the records about the Akaiwasha and the Danuna.

This last invasion, then, was probably a later wave of the migration which had begun about three centuries before. Because it does come at the end of the Helladic Bronze Age, it may be called the "traditional Dorian Invasion," although some of the traditions, I believe, must refer to the beginning of the migration, the one indicated by archaeology, namely, the "Achaïan" Invasion. My "Achaïan" and "Dorian" invasions, therefore, represent the first and the final waves of the great migration to the Peloponnesos of peoples of the Northwest Dialect group from the North. With the last or traditional "Dorian" invasion, the Late Helladic Period and the Bronze Age in the Peloponnesos come to an end.¹

IX. THE PELOPONNESOS IN THE BRONZE AGE²

(A Résumé of the Evidence and Conclusions)

I. THE NEOLITHIC AGE (previous to *circa* 2500 B.C.)³

Our knowledge of this period in the Peloponnesos is practically limited to the pottery and stone implements that have been found at a few sites.⁴ Judging by the pottery which has close affinities with towns built back from the coast are known as the Iron Age settlements. The fact that towns were destroyed about the same time over such an extent of territory, from Orchomenos and Thebes to Krete, makes more probable the theory that this last invasion was not a migration, but a raid by various bands from the North; perhaps some came by sea. If so, this would account for the fact that Krete suffered from the raid synchronously with, if not prior to, the mainland towns.

¹ I intend to rewrite and expand this section on the "Dorian Invasion" in the near future.

² In this section references will be given only to the preceding sections, except in cases where a new point or conclusion requires a particular note.

³ *V. supra*, sec. II, p. 7.

⁴ Gonia (*A. J. A.*, 1920, p. 7); Old Korinth (to be published by Dr. Walker); Hagioritika near Tripolis (*A. J. A.*, 1922, p. 469); stone implements found near Megalopolis (*Ἐφ. Ἀρχ.*, 1901, col. 85).

the early Thessalian wares I should class the Neolithic Peloponnesians with the peoples of a Northern culture group.

2. THE EARLY HELLADIC PERIOD (*circa* 2500 to 2000 B.C.)¹

As has been seen, there is archaeological, linguistic, and traditional evidence for the Helladic civilization in the first period of the Bronze Age. The main value of the archaeological evidence here seems to me to lie in the fact that it ties up these traditions and the linguistic evidence with the Early Helladic Period; and it also shows that in this early period the mainland's connections or relations were with the South, or, more correctly, with the islands. And so, in the light of this three-fold evidence, we may be justified in assuming that about the middle of the Third Millennium a branch of a non-Indo-European Anatolian race, the "Aigaians," akin to the Lykians of Asia Minor, spread over to the Peloponnesos. Here they settled and established their culture. The Neolithic Northerners (?) whom they found in possession of a few places were either driven out or assimilated.

The "Aigaians" lived in rectangular, flat-roofed houses made of sun-dried brick and wood, and used hand-made pottery of various kinds and shapes. But, most important of all, they introduced bronze into the Peloponnesos. They seem to have kept in touch with their kinsfolk on the Islands through a desultory trade-intercourse, but their culture naturally developed along different lines.

So far, traces of "Aigaian" or Early Helladic settlements have been discovered at Korinth and at ten other places in the Korinthia, at Zygouries in the valley of Kleonai, at Mykenai, Tiryns, Argos, Skoinochori (northwest of Argos), Lerna (Myli), Asine, Amyklai, Olympia, and on the island of Aigina.² And it is very likely that the spade will bring to light many more Early Helladic settlements in the Peloponnesos, as well as elsewhere in Hellas.

¹ *V. supra*, sec. III, pp. 8 sq.

² Korinthian sites (*A. J. A.*, 1920, pp. 1 sq.); Zygouries (*A. J. A.*, 1921, p. 298); Mykenai (*J. H. S.*, 1921, p. 265); Tiryns (*Ath. Mitt.*, 1913, pp. 88 sq., p. 341; Karo, *Führer*, etc., pp. 7 sq.); Skoinochori (*C. R. Acad. Inscr.*, 1922, pp. 100 sq.; *A. J. A.*, 1922, p. 101); Lerna-Myli and Asine (Wace and Thompson, *Prehist. Thessaly*, p. 224); Olympia (cf. *Korakou*, p. 13, fig. 17, and *Ath. Mitt.*, 1911, pp. 163 sq.); Aigina (note to appear in my *Prehistoric Aigina*).

The "Aigaians" seem to have maintained themselves and their civilization for several centuries in the Peloponnesos, until invaders from the North brought an end to their régime about the time of the beginning of the XIIth Dynasty in Egypt, i.e., *circa* 2000 B.C.

3. THE MIDDLE HELLADIC PERIOD (*circa* 2000 to 1400 B.C.)¹

The "Minyan Age"

As a result of the movement which brought the Indo-European peoples southward into Western Asia, the Balkan Peninsula, and the Italian Peninsula, the "Minyans" finally reached the Peloponnesos *circa* 2000 B.C. They were the first people of Indo-European stock to enter this region. Their invasion brought to a sudden end the civilization of the Early Helladic "Aigaians" whom they found in possession there. Previous to their arrival in the Peloponnesos a part at least of these "Minyans" seem to have settled a while in Phokis; and doubtless the people who continued to make Gray Minyan ware at Orchomenos were their kinsmen.

These invaders were, so far as we know, the first people of "Hellenic" or Indo-European stock to enter the Peloponnesos. Their dialect seems to have been that which is commonly called the "Arkadian," and for convenience these people may be called "Minyans." A leading, or what came to be a leading, cult was that of the sea-god Posoidan, who probably supplanted an Early Helladic marine deity, "Aigaïos" or "Aigaion." If such was the case, we have all the more reason for believing that the "Minyans" assimilated the conquered population, even if their invasion was of a cataclysmic character and brought about the destruction which is found in evidence on several sites.

The "Minyans" brought with them, I believe, the hoop-roofed curvilinear house; and out of this developed the megaron which was to remain the characteristic feature of the "Mykenaïan" palaces, and even of the temple architecture of classical times. We do not know when the "Minyans" began to build their houses without apses or curved lines, but undoubtedly their long stay in the Peloponnesos and their settled condition had caused them to give up the curvilinear

¹ *V. supra*, sec. IV, p. 12.

hoop-roofed house, the characteristic house-type of a migratory or nomadic people, and to adopt the more stable and permanent rectangular flat-roofed house. They could have adopted this style of house-construction from their subject "Aigaians," or could have learned it from the Minoans. That the change took place gradually is shown by the fact that at Korakou, of the four Middle Helladic houses discovered, two are apsidal and two are rectangular in plan.¹ These houses are to be dated in Middle Helladic I or II.

Another important contribution of the "Minyans" was wheel-made pottery, Gray Minyan ware, and the process by which this ware was made. This Gray Minyan was not only the most popular fine ware in the first century or two of "Minyan" domination in the Peloponnesos, but the typical Minyan shapes remained in vogue down through the Mykenaian Age.

Little can be said with certainty about the "Minyans" themselves apart from their development along artistic lines, and in addition to what we have said about their dialect and cult. However, it seems probable that they in time turned to the sea, and from trade with Krete and the islands acquired wealth and made possible the more rapid Minoanizing of the Helladic culture. In the later centuries of their régime, in Middle Helladic III and IV, the "Minyans" seem to have employed not only Kretan potters, but also Kretan artisans and artists. The mainland palaces point to this. It is not at all improbable that the "Minyans" made a raid on Krete in the latter part of Middle Helladic II or the beginning of Middle Helladic III. The palace at Knossos was rebuilt in the end of Middle Minoan III and again remodelled in Late Minoan I.² This would in part account for the increase of Minoan influence in Middle Helladic III. Kretan craftsmen may have been induced by the mainland chieftains with promises of rewards to come over to the Peloponnesos, or they may have come of their own accord, attracted by the wealth of the "Minyan" cities, especially Mykenai and Tiryns.

We know that the "Minyans" colonized the island of Kypros,³ but

¹ *Korakou*, p. 79.

² *V. supra*, n. 3, p. 24; Hall, p. 46.

³ Beside Kypros, the "Minyans" probably settled in Pamphylia and other places (Krete and other islands) where traces of the "Arkadian" dialect have been found. V. Kretschmer, in Gercke-Norden, I, p. 148. Since traces of Aiolic are to

whether this colonization was the result of the growth to excess of the population of the Peloponnesos and the consequent need of more land, or whether Kypros was colonized by "Minyans" who had been driven out of the Peloponnesos by the invaders who entered at the end of Middle Helladic IV, it is impossible to say with any degree of certainty. But Minoan or Mykenaian culture appears suddenly in Kypros, in Middle Helladic IV and in the Late Helladic Period, so that the island appears to have received a colony a little before or after the end of the fifteenth century. The colonization of Kypros is one event in the history of the "Minyans" for which we have proof.¹ I am inclined to believe that Kypros was colonized by the "Minyans" who had been forced to seek new homes because of the invasion of the Peloponnesos at the end of the Middle Helladic Period (*circa* 1400 B.C.). Kypros must have been known to them, because that island had been the chief supply-place for copper and bronze for centuries, and so it could have been one of the first of the islands, excepting Aigina and the neighboring islands, to be colonized by the "Minyans."

Probably the colonization of Kypros was synchronous with the destruction of the palace at Knossos. In this latter act I would see the hands of the "Minyans." This event is to be dated *circa* 1400 B.C., that is, at the end of Middle Helladic IV (LM II).² It is generally agreed that invaders from Hellas overran Krete and destroyed Knossos at this time. Opinions differ as to what people were responsible for the destruction of Knossos, but it is the consensus of opinion that they came from the mainland. The theory that they were the descendants of Kretan conquerors of the Peloponnesos and their subjects lacks all proof, since there is no sound evidence for a Kretan conquest of

be found in Kyrene (*v.* Gercke, *Hermes*, 1906, pp. 447 sq.), and there are traditions connecting Kyrene with Arkadia (*v.* Studniczka, in Roscher, *s.v. Kyrene*), "Minyans" may have eventually reached Kyrene.

¹ The connection between Arkadia and Kypros was recognized in antiquity, as the traditions show; *v.* Paus., 8, 5, 2.

² The palaces at Phaistos and Hagia Triada were also destroyed about this time, at least in LM III (Late Helladic Period); possibly also Gournia. The "megaron-type" of houses found at Hagia Triada and Gournia, which are to be dated in this period, were probably introduced by the "Minyans" (though possibly by the "Achaians"). *V.* Fimmen, pp. 52, 54, 210; for Gournia, H. Boyd-Hawes, *Gournia*; Beloch, I, 1, p. 127, n.; Fimmen, pp. 17, 54.

even the Argolid.¹ And that the "Achaïans," the people of the true Mykenaïan Age, the Late Helladic Period, overran Krete as early as 1400 B.C. is most improbable. For it is unlikely that the people who invaded the Peloponnesos at the end of the Middle Helladic Period would have passed on to Krete so soon after their arrival in the new land, which had to be conquered and held by force.

Is it not more probable that the people who destroyed the palace at Knossos and practically dealt the deathblow to the Minoan civilization in Krete were those "Minyans" who were compelled to seek new homes because of the invasion of the newcomers in the latter part of Middle Helladic IV?² This hypothesis is rendered all the more likely because the "Minyans" had to all appearances been in touch with Krete and the islands for centuries, and may have made a raid or raids on Krete sometime before. So it seems that history again repeated itself, and as the "Aigaians" were dispossessed of their land by the "Minyans," so the "Minyans" were forced to yield the greater part of the Peloponnesos to the second wave of "Hellenic" invaders. Some of the "Minyans" betook themselves into the mountain-hemmed interior of Arkadia, while others sought new homes across the sea, in Krete and Kypros, and doubtless in other islands.

4. THE LATE HELLADIC PERIOD (*circa* 1400 to 1100 B.C.)³

It was the "Achaïan" invasion that brought an end to the "Minyan Age" and régime of the "Minyans" in the Peloponnesos. This migration, which is dated by archaeology *circa* 1400 B.C., was made up of various tribes of the Northwest Dialect group who had come down from North Hellas, and had either crossed the Korinthian Gulf or had come into the Peloponnesos by way of the Isthmos. These migrating tribes, — among them were the Achaïoi, Myrmidons, 'Hellenes,'

¹ *V. supra*, p. 30.

² It was probably in this Late Helladic Period that the "Minyans" left the traces of their presence in Krete. Kretschmer (in Gercke-Norden, I, p. 156) gives the connecting ties between Arkadia and Krete: common place-names, a settlement Arkades in Krete, "Arkadian" dialectic traits; to these may possibly be added the suggestion that those who introduced the megaron-type of house to the Peloponnesos may have been the ones who constructed this type of house in Krete in LM III.

³ *V. supra*, sec. VII, p. 30.

Danaoi, Eleians, etc., — whom I have collectively called "Achaians," apparently came in great numbers, and their migration may have consisted of several waves and have covered one or more centuries.

Although the "Achaians" drove the "Minyans" back into the interior, perhaps there was greater assimilation of the conquered than has been supposed. Although they were of a lower culture-level than the Minoanized "Minyans," the "Achaians" do not seem to have been a barbarous, destructive people. They were undoubtedly warlike, but the "Achaian" invasion was not characterized by fire and the destruction of towns, as was the case with the "Minyan" invasion six centuries before. The "Achaians" do not seem to have made a practice of destroying towns. On the contrary, they took possession of the "Minyan" towns, built larger palaces, and constructed massive fortification walls. The plan and decoration of the Late Helladic palaces suggest the employment of Kretan (or Minoanized "Minyan") architects and artists.¹

The "Achaians" must have recognized the fact that the previous inhabitants were of their own race.² At Mykenai, when they built the akropolis circuit wall, by constructing a ring-wall of upright slabs, they preserved six shaft-graves in which were buried the rulers, or chieftains and princes, of the previous "dynasty."³ This may have been done for political as well as for religious reasons, for this act would win favor from the subject population who would hold the graves of the deceased rulers in great reverence.

Of the "Achaians" themselves we know very little. Their dialect, I believe, was that out of which developed the Dorian dialect of the classical period, and their chief deity was, in general, Zeus. From the fact that Achilles, Menelaos, Odysseus, etc., were called "yellow-haired" in the Epos, it is generally believed that the "Achaians" were a light-haired people. They seem to have introduced the fibula and the typical Late Helladic or Mykenaian figurines of the female deity into the Peloponnesos. Their architecture has been mentioned. The

¹ Cf. Holland, *Primitive Aegean Roofs*, in *A. J. A.*, 1920, p. 333.

² The tradition that the Dorians found a Hellenic people already in the Peloponnesos of course applies to the "Achaians," who might as well be called Dorians. As Beloch has shown, the term Dorian as an ethnic name did not come into general use until the eighth or seventh century (*v.* Beloch, I, 1, p. 141).

³ Wace, *J. H. S.*, 1921, p. 262.

"Achaian" palace was a mixture of Northern and Southern elements: the Northern megaron remained the principal feature, but the complex plan, flat roof, and decoration were for the most part Minoan in character. The Late Helladic palaces of the "Achaians" at Mykenai and Tiryns were apparently more Minoan than the palaces on these sites in the previous period. But the great circuit walls were introduced into the land by the newcomers, and the largest and more pretentious "Bee-hive" tombs were constructed by the "Achaians."

The "Achaian" invasion marked the beginning of a new era in pottery. As the invaders were people of a lower culture-level, it was but natural that an artistic decline should set in, and this is indicated by the deterioration in technique and decoration. The appearance of the new shapes is rather an indication of a change in artistic taste, or in culture, than an innovation brought from the invaders' former home.

The "Achaians" kept up the trade-relations that had existed between the mainland and the islands from Early Helladic times. A closer connection with Krete seems to have developed, and one would naturally expect this; all the more, since Krete, or a part of the island, was probably in the possession of Hellenic people in the Late Helladic Period (LM III).¹ I have suggested that "Minyans" overran Krete and destroyed the palace at Knossos *circa* 1400 B.C., since I do not believe that "Achaians" could very well have had time to conquer the Peloponnesos and arrive in Krete so early. Lakonia does not seem to have been Doricized (i.e., by "Achaians") until a considerable time after the Argolid. But toward the end of the Late Helladic Period the "Achaians" apparently began to overflow the Peloponnesos, and to colonize Krete, Rhodes, and the other islands. And, as we have seen, the Akaiwasha and the Danuna, who took part in expeditions against Egypt *circa* 1230 and 1196 B.C. respectively, were quite probably "Achaians" from the Peloponnesos.

It is to be noted that the "Achaians" did not settle in the northern part of the Aegean. They colonized the Kyklades and lands of the South Aegean.² The post-Mykenaian colonies which went out from

¹ V. notes on pp. 55, 56.

² The so-called Dorian colonies, or Doris. Rhodes seems to have been colonized towards the end of the Late Helladic Period. Cf. Fimmen, pp. 16 and 211.

the Peloponnesos to southern Asia Minor and the South Aegean islands (i.e., to Asiatic Doris) were probably following in the steps, or tracks, of their Helladic or Mykenaian ancestors. Hence I do not believe that the wealth of the Argolid cities was derived from trade with North Aegean lands, nor do I believe that Mykenai, much less Sparta, had any historical relations or connections with Troy. The Sixth City at Troy did yield Minoan finds (they were called "Mykenaian" before the Kretan discoveries); but this certainly does not indicate any direct relations with the Argolid, for by this time the Minoan culture had spread to many lands. The tradition that brings Peloponnesian "Achaians," or any people from the Peloponnesos, to Troy in the Bronze Age does not seem to me to have any historical foundation. The people who captured and destroyed the Sixth City at Troy were the people who settled in the North Aegean islands and the Troad;¹ they were Aiolic colonists from Thessaly, for whose interest it was necessary that Troy be destroyed. Hence it seems that the most famous event ascribed by "Homer" and tradition to the Peloponnesian "Achaians" must be taken away from them, and be given their former Aiolic neighbors in Thessaly.

The greatest objection to the theory that the "Achaians" spoke Doric is that in such a case we should have an Aiolic-Ionic epic about a people that was neither Aiolic nor Ionic. But possibly the homeland of the "Achaians" had been Aiolic before the Northwest Dialect group had taken over the land; and while the "Achaians" were still in North Hellas, they were neighbors to Aiolic peoples. That Aiolic saga should deal mostly with non-Aiolic "Achaians" is not so strange. There are parallels. The Anglo-Saxon "Beowulf" is localized around Denmark, and the kernel of the French "Chanson de Roland" is Germanic.² Besides, if Ionians could take over Aiolic saga, how can one logically object to the theory that the Aiolians in their homeland had appropriated "Achaian" lays and names from their neighbors?

¹ The Sixth City may have actually been destroyed by Phrygian invaders from Europe; but it is the wars fought between the Thessalian colonists and the inhabitants of the Troad (and of Troy) that form the historical background of the Epos.

² Cauer, *Grundfragen der Homerkritik*, I, pp. 162-163. Cauer, however, believes that the dialect of Phthiotis, and likewise that of its inhabitants, the Achaians, was the Aiolic.

The Late Helladic Period appears to have been one of movements, and there were probably several waves of Northwest Dialect peoples. To these may have been due in part the colonizations and expeditions that took place in this period. The traditional "Dorian Invasion," which, I believe, was the last wave of the "Achaians," appears to have been the last invasion of the Peloponnesos. It was of a more violent and destructive nature, but I do not think that it brought an end to the "Achaian" régime in the Peloponnesos. That lasted as long as the Spartans or other Dorian states remained supreme.¹ And the growth of the name and fame of the 'Hellenes' argues for their continued existence in power down into the strictly-speaking "historical" period.

The chief importance of the last wave of the migration of the (Northwest Dialect) "Achaians," the traditional "Dorian Invasion," lies in the fact that it brought an end to the Bronze Age, for it was these last invaders who introduced iron into the Peloponnesos. While the "Dorians" brought destruction to many "Achaian" cities, still they cannot be said to have brought on the "Dark Age." In the first place, they do not seem to have had any permanent effect on the people whom they raided. To be sure, art continued to decline, but the decline had already started during the "Achaian" régime, and it naturally continued. But it is held that Geometric pottery marks an advance in ceramic technique. Then, too, the "Dorians" apparently did contribute something that revolutionized daily life as well as warfare, and that was the secret of making iron.

On the whole, it does not seem necessary to believe that a "Dark Age" followed the Mykenaian Age. The early centuries of the Iron Age may be dark merely because we lack information about this period. The decline along artistic lines may have been balanced by a rise politically and commercially. This period, however, lies beyond the limits set for this paper; and so, with the "Dorian Invasion" at

¹ It is to be noted that the Epos does not give any indication of a radical change from an Achaian to a non-Achaian régime in the Peloponnesos. The passage in Plato's *Laws* (3, 682 E) shows that not everyone in the classical period made a distinction between Achaian and Dorian, as regards the Peloponnesians. As Kahrstedt, following Beloch, has said, later writers thought that a different people must be implied, because the name Achaioi was no longer current (*Neue Jahrbücher*, 1919, p. 72).

the end of the Late Helladic Period (*circa* 1100 B.C.), I bring to a close this outline of a History of the Peloponnesos in the Bronze Age.¹

X. PELOPONNESIAN CHRONOLOGY (BRONZE AGE)

Circa 2500 B.C. The NEOLITHIC AGE ends, and the BRONZE AGE begins.

Circa 2500–2000 B.C. The EARLY HELLADIC PERIOD.

The non-Indo-European "Aigaians" of Anatolian stock inhabit the Peloponnesos.

Circa 2000 B.C. The "Minyan Migration." The "Minyans," the first of Hellenic or Indo-European stock to enter the Peloponnesos, bring in Gray Minyan ware, the curvilinear hoop-roofed house, the "Arkadian" dialect, etc.

Circa 2000–1400 B.C. The MIDDLE HELLADIC PERIOD.

The "Minyan Age," the period of "Minyan" régime in the Peloponnesos.

Circa 2000–1800 B.C. MIDDLE HELLADIC I. Synchronisms with Middle Minoan II and the XIIth Dynasty in Egypt.

Circa 1800–1600 B.C. MIDDLE HELLADIC II. Minoan influence begins to appear on the mainland. In latter part of this period, first interment of Sixth Shaft-Grave at Mykenai. Possibly "Minyans" make a raid on Krete.

Circa 1600–1500 B.C. MIDDLE HELLADIC III. Synchronisms with Late Minoan I and the XVIIIth Dynasty in Egypt. Increase of Minoan influence. Possible raid by "Minyans" on Krete. Shaft-graves at Mykenai continue, and receive last interments at end of period. First "Bee-hive" tombs date at end of this period. Early palace of Mykenai style (mixture of Northern and Southern elements) at Mykenai probably of this period.

Circa 1500–1400 B.C. MIDDLE HELLADIC IV. Early palace at Tiryns in this period (if not in MH III). Kakovatos "Bee-hive" tombs. Zenith of "Minyan" culture probably in this period. "Ephyraean" ware illus-

¹ This paper is the result of an expansion of a section of my Doctor's Dissertation, *Prehistoric Aigina*, the revision of which is about completed. In the foregoing pages I have attempted to bring together the traditions, the linguistic evidence, and the results of the recent archaeological investigations. My apology for the presence of hypotheses in this history is that in dealing with this early period one must have recourse to hypotheses (but, of course, fact and hypothesis must be distinguished from each other). Some hypothesis may remind another of evidence that has not been taken into account in its proper connection, or it itself may even be confirmed by new evidence in the future. It was with this idea in mind that I ventured to advance some of these theories.

trates the fusion of "Minyan" with Minoan culture, which produced the "Mykenaian" culture. Island of Kypros colonized by "Minyans" in latter part or at end of this period.

Circa 1400 B.C. The "ACHAÏAN" INVASION.

"Minyan" régime in the Peloponnesos brought to an end by an invasion of tribes of the Northwest Dialect group, the "Achaïans." "Minyans," driven back into the interior, remain independent. Krete overrun and palace at Knossos destroyed, perhaps by the "Minyans."

Circa 1400-1100 B.C. The LATE HELLADIC PERIOD.

Period of "Achaïan" or "Achaïo-Dorian" régime. The true Mykenaian Age. Synchronisms with the reign of Akhenaten at Tell-el-Amarna in Egypt. New palaces at Mykenai, Tiryns, etc. The "Cyclopean" walls at various sites — Mykenai, etc. The great "Beehive" tombs. The Grave Circle enclosing the earlier Shaft-Graves constructed. Rhodes and other islands colonized by "Achaïans."

Circa 1230 B.C. The Akaiwasha, "Achaïans" from the Peloponnesos or the South Aegean islands colonized from the Peloponnesos, take part in an expedition by sea against Egypt, and are defeated by Menepthah.

Circa 1196 B.C. The Danuna, identified with the "Achaïan" Danaoi and presumably from the Peloponnesos or lands colonized therefrom, along with other tribes taking part in an invasion, are defeated in Palestine by Rameses III.

Circa 1100 B.C. The traditional "Dorian Invasion"; the last wave of the migration of the "Achaïans," the peoples of Northwest Dialect group. The Peloponnesos suffers a destructive raid at the hands of warlike, uncultured tribes who may have been a branch of the same stock as the "Achaïans"; some may have come by sea. About the same time Krete coastal towns and Boiotian Orchomenos and Thebes are destroyed. The destruction of Mykenai and Tiryns marks the end of the Mykenaian Age and of the Late Helladic Period.

Circa 1100 B.C., or the eleventh century B.C. Introduction of Iron into the Peloponnesos, and the END of the BRONZE AGE.

THE ΤΗΟΖΩΜΑΤΑ OF ANCIENT SHIPS

BY FRANK BREWSTER

MR. CECIL TORR in his work on ancient ships¹ makes the following statement: "On the war-ships the hull was strengthened externally by a set of cables. These were known as *hypozeugmata*, or girdles, and used to be fastened round the ship horizontally; the two ends of each cable being joined together, so as to make it a complete girdle extending from stem to stern along the starboard side and back from stern to stem along the port side." This proposition has been disputed by Mr. E. G. Schaubert² on the ground that such use of a rope would be of no practical value. There is undoubtedly much force in this argument; and I note that Mr. Rice Holmes³ has a memorandum of a conversation with a sailor, who expressed an equally positive opinion on the uselessness of such a contrivance. On the other hand, the same note shows the use of chain-cables in just such a way as Mr. Torr maintains; and the mate of a steamer, whom I asked about this last summer, told me that he had seen chains used in this way on old Norwegian boats. He also told me that he thought a rope might be used for the same purpose, if it were drawn tight by twisting. It would not seem possible, therefore, to dismiss Mr. Torr's theory on the ground of inutility only, though perhaps the fact that this has been questioned renders the theory less probable.

When, however, we come to examine the evidence cited by Mr. Torr in his notes in support of his statement, it seems insufficient to justify his assertions. The first passage he cites is Athenaeus, *Deipnosophistae* 5, 37:

τὴν τεσσαρακοντῆρην ναῦν κατεσκεύασεν ὁ Φιλοπάτωρ τὸ μῆκος ἔχουσιν διακοσίων ὀγδοήκοντα πηχῶν, ὀκτὼ δὲ καὶ τριάκοντα ἀπὸ παρόδου ἐπὶ

¹ *Ancient Ships*, Cambridge University Press, 1894, p. 41.

² "The *ὑποζώματα* of Greek Ships," *Harvard Studies in Classical Philology*, XXII, p. 178.

³ *The Classical Quarterly*, III (1909), p. 34, n. 3.

πάροδον, ὕψος δὲ . . . ὑποζώματα δὲ ἐλάμβανε δώδεκα· ἑξακοσίων δ' ἦν ἕκαστον πηχῶν.

The only positive evidence contained in this passage is that this particular ship had 12 ὑποζώματα, each 600 cubits long. It is, of course, true that twice the given length of the ship plus the given breadth is almost exactly the same as the given length of the ὑποζώματα; but Mr. Torr's statement that each cable "would be just long enough to pass once round the ship from stem to stern" seems to assume more than the evidence warrants. On page 22 Mr. Torr refers to the long overhangs at bow and stern, and his plates show similar construction; There would be considerable difference, therefore, between the length over all and that at the waterline. It does not appear whether the length given in the quotation was length on the waterline or over all. but as the cable would probably run at some intermediate point, the exact equivalence of the measurements would no longer exist. At the most, this evidence seems merely consistent with Mr. Torr's theory, not proof of it.

The next passage cited is from the earlier Athenaeus, the author of the work *De Machinis*:¹

ὑποζώννυται δὲ ὅλος ὁ κριὸς ὅπλοις ὀκταδακτύλοις τρισί, καὶ διαλαμβάνεται κατὰ μέσον ἐκ τριῶν διαλειμμάτων ἀλύσει πηχναίαις.

A battering-ram was essentially a long heavy stick of timber mechanically supported so that it could be swung back and forth against a wall by man power. Now three ropes eight fingers thick running round a long beam from point to end would not seem to add anything to its strength; but used as trusses they would render it more rigid. At least three ropes would be necessary for this purpose, so as to balance each other. They could easily be made taut by props placed between each rope and the corresponding face of the timber, and pounded into position. The chains a cubit long placed at three intervals in the middle would be needed to keep them in position. So far as this evidence goes, it looks as if the word ὑποζώννυται was used here with something of the same meaning as our word "trussed."

¹ *De Machinis*, ed. C. Wescher, *Poliorcétique des Grecs*, Paris, 1867, p. 24.

The next passage cited is Vitruvius 10, 15, 6. In Rose's second edition, 1899, this passage is given as follows:

A capite autem ad imam calcem tigni contenti fuerant funes III crassitudine digitorum VIII, ita religati, quemadmodum navi a puppi ad proram continenter, eique funes praecinctura transversa erant ligati habente inter se palmipedalia spatia.

Morgan's translation, which follows this text, reads as follows:

From the head to the very heel of the beam were stretched cables, three in number and eight digits thick, fastened just as in a ship from stem to stern continuously, and these cables were bound with cross girdles a foot and a quarter apart.¹

This is substantially the same statement as that contained in the passage from Athenaeus, *De Machinis*, but gives the further information that the ropes in question were fastened as on ships from stem to stern. Mr. Torr refers to this as proving that the ὑποζώματα ran from stem to stern, and with this Mr. Schauroth² apparently agrees so far as the Latin writers are concerned. It is to be noted, once more, that winding either three or four ropes eight digits thick round a long stick of timber, lengthwise, would serve no practical purpose; while used as trusses they would increase its rigidity. With a square hewn piece of timber, four ropes would be the natural number to use, one for each face. The cross-ties would be needed to keep the trusses from collapsing sidewise. Here again the evidence seems to point to the use of the ὑποζώματα on board ship for the purpose of trusses.

Next follows a citation from Plato's *Republic*, 616 C,³ which, Mr.

¹ M. H. Morgan, *Vitruvius, The Ten Books on Architecture*, Harvard University Press, 1914, p. 314. The present passage occurs in the section translated by A. A. Howard: see Preface, p. iii. The latest edition, by Krohn, 1912, reads: *quemadmodum naves a puppi ad proram continentur eique funes praecinctura e transversis erant religati habentes inter se palmipedalia spatia*. This is a return to the reading of the MSS., the meaning of which is essentially that of Howard's translation, except for *habentes*. Here the emendation to *habente* seems necessary, for the foot-and-a-quarter distances obviously apply to the girdles rather than to the cables. Torr's text, as that of Rose in his first edition (1867), retains *funes IIII* of the MSS., which Rose in his second edition emended to *funes III* on the strength of the passage in Athenaeus. But perhaps we can dispense with this conjecture. Vitruvius might well have known of rams that had four cables.

² *Op. cit.*, p. 179.

³ καὶ ἰδεῖν αὐτοῖσι κατὰ μέσον τὸ φῶς ἐκ τοῦ οὐρανοῦ τὰ ἄκρα αὐτοῦ τῶν δεσμῶν

Torr says, shows that the ὑποζώματα went round the ship externally. This passage is more fully quoted by Mr. Schauroth, who maintains that the beam of light did not hold the heavens together by passing around them, but by passing through them like the diameter of a sphere.¹

In regard to this citation, two points are to be noted. First, Plato evidently refers to the ὑποζώματα of the triremes as a device well-known to his readers, and as an appropriate illustration of his own meaning. If Plato thought his own statement needed clarifying by an illustration, it is evident that without the illustration it cannot be perfectly clear how Plato conceived that the beam of light acted as a bond in holding the heavens together. Second, so far as we can understand Plato's conception, Mr. Schauroth's interpretation is more probable than Mr. Torr's. The observers saw both the earth and heavens, therefore they could not have seen that portion of the bond, if there were such, which went around on the outside of the heavens. Moreover the words κατὰ μέσον which Plato uses would seem to indicate that the light extended through the middle, which is just the definition of a diameter and not at all the conception of an encircling bond.

Mr. Torr also refers to the Athenian dockyard inventories, which indicate that the ὑποζώματα were made of rope. This I understand is not now questioned.

The results of this evidence may be summed up thus. The use of the technical term in connection with battering-rams would seem to indicate, first, that the ὑποζώματα formed some kind of truss; second, that on ships they ran from bow to stern; third, that it is more probable that they ran straight through the middle of the ship than round on the outside.

Now, as a matter of fact, there is positive evidence that ropes were used exactly in this way and for exactly this purpose. In Plate I, figs. 4 and 5 at the end of his book, Mr. Torr reproduces drawings of two Egyptian merchant ships of about 1250 B.C., on each of which we see a large rope running from bow to stern over a series of props, and

τεταμένα· εἶναι γὰρ τοῦτο τὸ φῶς ξύνδεσμον τοῦ οὐρανοῦ, οἷον τὰ ὑποζώματα τῶν τριήρων, οὕτω πᾶσαν συνέχον τὴν περιφορὰν.

¹ *Op. cit.*, p. 177.

apparently attached to smaller ropes wrapped round the fore and aft overhangs. Mr. Torr rightly says of these ropes that they "were intended to prevent the ship from hogging," but he gives no authority for his additional remark "and would have been superfluous on Greek or Roman war-ships, which had decking enough to hold the stem and stern together." As to the last point, Mr. James Smith, in his *Voyage and Shipwreck of St. Paul*,¹ writes:

The next quotation is from Isidore, and is more to the purpose, because it does appear that ropes were occasionally applied in a longitudinal as well as a transverse direction, to prevent ships from straining. "The *tormentum* is a cable in long ships, which is extended from stem to stern, in order to bind them together."² Isidore mentions two kinds of cables for the purpose — the *mitra*, to bind them round the middle, and the *tormentum*; this, he says, is so called because it is twisted. There is nothing which implies that it was passed round the ship externally; and it is not clear how a ship could be bound together in the mode supposed: the *naves longae*, from the weight of the rostra and towers at the extremities, and from their great length, must have been extremely apt to "hog," or fall down at each end; but as the stem and stern posts rose above the rest of the vessel, a simple way of preventing this would be to pass a rope round them, and heave a strain upon it by twisting the parts together, as was done in the military engines called *tormenta*; and Isidore's etymology of the name *tormenta*, a *tortu dicta*, seems to confirm this.

Here, then, is an interesting diversity of opinion. The important point, however, is that we have in the pictures of the Egyptian ships positive evidence that about 1250 B.C. a rope-truss was used to support the forward and aft overhangs and keep them in position. "Hogging" is merely humping up in the middle; and one method of preventing it is by the use of a truss to support the two ends of the boat and thus keep her in shape.

That the use of a rope for this purpose was an old device is shown by another picture of an Egyptian boat, to be found in Breasted's *Ancient Times*.³ Here again we see a rope or ropes running from stem to

¹ Third edition, pp. 206 ff.

² Isidore, *Etym.*, ed. Lindsay, Oxford (1911), 19, 4, 4: *Tormentum funis in navibus longus, qui a prora ad puppim extenditur quo magis constringantur. Tormenta autem a tortu dicta.* The text used by Smith evidently had *longis* for *longus*.

³ James Henry Breasted, *Ancient Times, a History of The Early World*, 1916, p. 58, fig. 41. A larger reproduction may be found in Borchardt, *Das Grabdenkmal*

stern over a series of props. Mr. Breasted states that this was a ship of the twenty-eighth century B.C. The beautiful lines of the hull of this boat closely resemble those of a modern yacht. I have seen many boats at Marblehead with just such sheer and overhangs. It is hardly to be supposed that shipbuilders who could design crafts with such modern lines would have installed a heavy rope running from stem to stern, right down the middle of the deck, if it had not served a necessary and useful purpose. As a matter of fact, a truss involving the same principle is used on river steamers, and I have seen a racing boat of light construction and long overhangs use a similar truss amidships, only in this last case it was made of iron rods.

The pictures in Mr. Torr's book do not show clearly how this rope-truss was drawn taut; but the picture of the twenty-eighth-century boat solves this difficulty. In the middle there is clearly a bar, passing through, or rather, as I should think, between, two ropes, which have evidently been twisted together by means of the bar, both ends of the bar being lashed to keep it from untwisting. This would seem to indicate that the rope-truss was made of a rope passing forward and back through the fastenings fore and aft, and then drawn taut by twisting the two parts together by a bar run between them. This is almost exactly Mr. Smith's explanation, quoted above, of Isidore's definition of the *tormentum*. It does not seem to me probable, however, that the *tormentum* was passed round the projections at bow and stern. The natural fastening would be under the keel, or to the keel at the bottom fore and aft. The Egyptians apparently obtained this result by passing ropes around each overhang and attaching the rope-truss to them. In the later construction, when metal was more common, some other device for carrying the pull down to the keel may have been adopted; or the large rope may have actually passed below the deck to an attachment on the keel itself. The length of the *ὑποζώματα* on Philopator's ship, in the passage first cited,¹ would seem to indicate this. With a ship 280 cubits long, to pass a double rope from an attachment forward on the keel over a series of props to another attachment on

des Königs Saḥu-Rē Band II, *Die Wandbilder, Abbildungsblätter, Blatt 13*. (26. Wissenschaftliche Veröffentlichung der deutschen Orient-Gesellschaft, Leipzig, 1913.)

¹ See above, p. 63.

the keel near the stern would require just about 600 cubits. We need not assume that all twelve cables were used at once. Some, perhaps most of them, were spares. Mr. Torr¹ says that the Athenian war-ships carried two or three; and this looks as if each had one or more spares. Of course some spares would be needed as there must have been always a likelihood that the ropes would break under the strain.

But this is not all. In the *Argonautica* of Apollonius Rhodius, I, 367 ff., we read:

νῆα δ' ἐπικρατέως Ἀργου ὑποθημοσύνησιν
ἔζωσαν πάμπρωτον εὐστρεφεῖ ἐνδοθεν ὄπλῳ
τεινόμενοι ἐκάτερθεν, ἔν' εὖ ἀραροίατο γόμφοις
δούρατα καὶ ῥοθίου βίην ἔχοι ἀντιώσαν.

These lines are translated by Mr. Seaton:²

First of all, by the command of Argus, they strongly girded the ship with a rope well twisted within, stretching it tight on each side, in order that the planks might be well compacted by the bolts and might withstand the opposing force of the surge.

As the twist in a rope is continuous through its whole diameter, ἐνδοθεν would seem to refer to the boat itself rather than the rope; and the passage would mean that the rope was well twisted inside the boat.³ This is in exact agreement with the rope-truss we see depicted in the pictures of the Egyptian boats. Ἐκάτερθεν is translated by "on each side," but it may just as appropriately refer to bow and stern. Ἐζωσαν is translated "girded," but it may well be that this word not only meant "wrapped round the middle" but was used in a technical sense, as Mr. Schauroth, page 175, suggests of ὑπόζωμα, like our word "girder."

If the rope was twisted up inside the boat it could not have passed round her hull longitudinally on the outside; and it is difficult to see how it could have passed over and around the middle of the hull. The

¹ *Op. cit.*, p. 42.

² *The Loeb Classical Library*, 1919, p. 29.

³ Edmond Warre, "On the Raft of Ulysses," *Journal of Hellenic Studies*, V (1884), p. 216, apparently accepts this view and so also does Mr. Schauroth, *op. cit.*, p. 176. Mr. Torr, *op. cit.*, p. 42, n. 102, makes no comment on ἐνδοθεν. Sanctamandus emended this to ἐκτοθεν. On the notes of this scholar, see the edition of Apollonius by John Shaw, 1779, *praef.*, p. [3].

Argo was lying on the beach and it would have been impossible to gird her with a rope wrapped round the hull without digging a trench under the boat or propping up the hull on blocks. Neither of these acts is mentioned, and the fact that the poet immediately passes on to tell how a trench was dug under the bow so as to place rollers under it, would seem to indicate that nothing of this sort was done. Certainly, if it was customary to dig a trench under a boat on the beach in order to place a rope round her hull, the poet, who tells us how the rolls were placed to facilitate launching, would have been likely to mention the former operation. This reasoning, of course, is not conclusive; but the description of an act which fits so well with what we see in the Egyptian ship is more likely to refer to that than anything else. None of the pictures of Greek and Roman ships, shown on Mr. Torr's plates, give any indication of any rope used for strengthening the hull such as we see on the pictures of the Egyptian boats. This might be due to the fact that these ropes were only put in use in bad weather. The lines he quotes from Horace¹ are in keeping with this. Mr. Smith, page 207, expresses the opinion that ἐλάμβανε, in the passage from the *Deipnosophistae*² means, not that the ὑποζώματα were in place, but only that the ship carried them. In *Acts* 27, 17, we read that on the approach of the storm the sailors on St. Paul's ship, "βοηθείαις ἐχρῶντο ὑποζωννύντες τὸ πλοῖον." This certainly looks as if the ὑπόζωμα of this ship had not previously been in use.

This passage from *Acts* seems to confirm the theory that the ὑποζώματα were the same as the Egyptian rope-truss. Mr. Smith remarks (pages 20ff.):

As a voyage-writer St. Luke is possessed of another most essential qualification, he is thoroughly versant in nautical matters, and describes them in the appropriate language of seamanship. . . . But although his descriptions are accurate, they are, as I have already observed, unprofessional. . . . The impression produced by incidents at sea upon the mind of the passive observer is altogether different, and of course his mode of describing them equally so. He tells us what has happened, but rarely tells us how or why the measures connected with it were taken. In doing so he often mentions circumstances which a seaman would not think of noticing from their famil-

¹ *Carm.* I, 14, 6 f. Ac sine funibus | vix durare carinae | possint imperiosius | aequor.

² See above, p. 63.

iarity, or from being matters of course. . . . His notices of events are altogether accidental and fragmentary. He records them simply because he observes them, not because they are intrinsically important.

With these characteristics in mind turn back to the passage above quoted. *Βοηθείαις* means, among other things, "supports." This word seems quite appropriate to the props under the rope which we see on the Egyptian ships. If the ὑπόζωμα had not been in use, the first act of the sailors would have been to set up the props, and then run the double rope over them and through its fastenings fore and aft, and twist it up. This is all the writer would have seen done and all he has in fact reported. His narrative tallies with such a device as we see on the Egyptian ships, but with nothing that we can in fact imagine if the ὑπόζωμα girded the ship externally. Indeed Mr. Torr admits (page 42, note 102) that his kind of ὑπόζωμα could not have been set up except in a dockyard.

Mr. Torr further says that as the ship was a merchant-ship, it did not carry an ὑπόζωμα and that the captain therefore used some expedient for it. This seems improbable; the writer uses the technical term, and the only fair inference is that he meant the real technical device. The fact that Plato, Athenaeus and Vitruvius all refer to the ὑποζώματα, or the corresponding verb, as an illustration of another device, shows that its general character was well known. There is no reason why the writer of *Acts* should not be credited with what was more or less common knowledge. If we accept Mr. Smith's conclusions (he cites the use of at least fourteen special nautical terms),¹ there can be little doubt that the writer used the technical term to express the technical act.

If now we turn back to the passage cited from Plato, and read it with what immediately precedes, it seems to me we may obtain a fairer conception of his meaning. Jowett's translation, III, page 338, reads:

He said that they came to a place where they could see from above a line of light, straight as a column, extending right through the whole heaven, and through the earth, in colour resembling the rainbow, only brighter and purer; another day's journey brought them to the place, and there, in the midst of the light, they saw the ends of the chains of heaven let down from

¹ *Op. cit.*, pp. 27 f.

above: for this light is the belt of heaven, and holds together the circle or the universe, like the undergirders of a trireme.

From this passage we note certain facts. First, the light itself is the bond, not the chains let down from heaven. Second, the light is straight like a column and extends through the whole heaven and the whole earth. Third, there are in the light the ends of the chains of heaven let down from above. Do we not have here exactly what we see in the Egyptian pictures? The light is the bond. So the rope-truss is the bond binding together the two ends of the ship. The light is straight as a column and extends through the whole heaven and the whole earth. So also the rope-truss runs straight through the whole ship from bow to stern. At its end the chains of heaven are let down into its midst. So, too, the rope-truss meets at bow and stern the encircling ropes by which its tension is passed under the keel holding the whole circumference of the ship in shape. Indeed, may we not see in Plato's chains the means used on the triremes to act as the attachments for the rope-truss, the *ὑπόζωμα* of the trireme? Why should Plato speak of the "chains of heaven" if this was not exactly the means for this attachment on the triremes?¹

The remaining passages cited by Mr. Torr² are, first, Appian, *De Bellis Civilibus*, 5, 91:

ὁ δὲ (Πομπήιος) οὔτε περὶ τῆς γῆς ἐνενόησεν, οὔτε τοῖς λειψάνοις τοῦ ναυαγίου παροῦσιν ἢ ἀπιοῦσι καταστάντος τοῦ κλύδωνος ἐπεχείρησεν· ἀλλ' ὑπερείδεν ἐκ τῶν δυνατῶν διαζωννύμενος τὰ σκάφη, καὶ ἀνέμῳ διαπλέοντας ἐς τὸ Ἰππώνειον.

From the passage in the *Argonautica* it is evident that one of the first steps in preparing a ship for sea was to set up the *ὑποζώματα*. Hence this act may well have come to typify the preparations necessary for fitting out and was used here in this sense. At all events, it seems plain that this passage does not give us any information as to what the *ὑποζώματα* were.

¹ Warre, *J. H. S.*, V., p. 217, after referring to the Egyptian rope-truss, writes: "Possibly the difficulty about the *ὑπόζωμα*, Plat. Rep. Bk. X, finds its solution in this straight truss amidships." See the note on 616 c, in Jowett and Campbell's edition of the *Republic*, III, p. 473.

² *Op. cit.*, p. 42, n. 102.

Nor is the quotation from Polybius 27, 3, 3, given by Mr. Torr any more explicit: ¹

καὶ τεσσαράκοντα ναῦς συμβουλευσας τοῖς Ῥοδίοις ὑποζωννύνειν, ἵν' ἐάν τις ἐκ τῶν καιρῶν γένηται χρεία, μὴ τότε παρασκευάζωνται πρὸς τὸ παρακαλούμενον, ἀλλ' ἐτοίμως διακείμενοι πράττωσι τὸ κριθέν ἐξ αὐτῆς.

The object of the advice was apparent. The Rhodians were to keep forty ships in a state of readiness, so that on being needed they could at once put to sea. But much more is necessary to fit a ship to go to sea than merely putting on the ὑποζώματα, whatever they may be. The ship's hull must be looked over, her bottom cleaned, the ship made tight. Masts, sails, spars, oars and all the rigging and extra parts must be on board. Therefore ὑποζωννύνειν perhaps does not denote merely the putting on the ὑποζώματα; it may indicate the other necessary preparations for putting to sea.² At all events, this passage throws no further light on the nature of ὑποζώματα.

This exhausts all the evidence Mr. Torr cites on the question. There is, however, one further bit of information which Mr. Smith affords. On page 205, he cites the following from Hesychius: Ζωμεύματα, ὑποζώματα σχοινία κατὰ μέσον τὴν ναῦν δεσμενόμενα. Mr. Smith translates κατὰ μέσον by "round the middle"; but this is certainly not the only possible, or even the most probable, meaning of the expression. Liddell & Scott give as one of the meanings of κατὰ with the accusative "on, over or throughout a space"; and Professor Herbert Weir Smyth³ says of κατὰ, "With the genitive, the motion is perpendicular; with the accusative, horizontal." Now the motion of a rope passed round the hull of a ship is perpendicular, while that of a rope running from bow to stern as shown in the Egyptian pictures is really horizontal. Our own expression "down the middle," which would be a literal translation of κατὰ μέσον, seems to me an exact description of the Egyptian rope-truss; and Hesychius's definition,

¹ I cite from the edition of Büttner-Wobst, Leipzig, 1904, IV, p. 189.

² Evelyn S. Shuckburgh, in his translation of Polybius (London, 1889), renders the above passage as follows: "and, among other things, had counselled them to repair forty ships, in order that, if any occasion for using them should arise, it should not find them still in the midst of preparations, but ready to answer to the call and to carry out their resolve at once."

³ *A Greek Grammar for Colleges* (1920), § 1690.

therefore, should be regarded as pointing to this rope-truss, rather than to a cable passed around the hull amidships.

It does not follow, of course, that Greek and Roman sailors never used a rope passed around the hull to strengthen their ships. Isidore¹ defines *mitra* as *funis qua navis media vincitur*, and in the Biblical epic *De Iona*, 38 f., we read: ²

Tunditur (navis) hinc illinc tremit omnis silua sub ictu
Fluctifrago, subter concussae spina carinae
Palpitat, antennae stridens labor horret ab alto,
Ipse etiam infringi dubitans inflectitur arbor.
Nauticus interea gemitus timor omnia temptat
Pro rate proque anima: spiras mandare morantis,
Oblaqueare mithram, clauorum stringere nisus,
Vellere luctantes, impellere pectore gyros.

These passages seem to leave no reasonable doubt that the Roman seamen, at least, used a rope girdle for strengthening a ship, but they do not show where it was placed. *Media* does not necessarily mean amidships, but rather somewhere round an important part of the hull. The most effective service of an encircling cable would be in keeping the outside planking in position. This was obviously the purpose for which they were used in the modern instances cited by Smith.³ The planking would be most likely to start near the bow or stern where its greatest curvature occurs. Now the ropes round the fore and aft overhangs, which we see on Torr's plates, are well located to serve this purpose in addition to acting as attachments for the rope-truss. Indeed the tension of the truss would be very effective in keeping such girding ropes taut, and so holding the planking in place. The greater the strain on the truss the more firm the grip on the planking. It may well be, therefore, that the *mitra* was nothing but the name — and a very appropriate name — for these encircling ropes. If so, we have in Isidore's definitions of the *tormentum* and *mitra* both kinds of ropes which we see on the Egyptian ships.

This suggestion, however, cannot be regarded as more than a reasonable possibility. The evidence merely shows that the *mitra* encircled

¹ *Etym.* 19, 4.

² Cypriani *Opera Omnia*, ed. Hartel in *Corp. Script. Eccles. Lat.* III (3, p. 299), 1871.

³ *Voyage and Shipwreck of St. Paul*, pp. 102 ff., esp. 108.

the hull somewhere, and may have been amidships. Nevertheless it does seem clear that the *mitra* was not the equivalent of the Greek ὑποζώματα.

Liddell & Scott's Lexicon gives *tormenta* as the Latin equivalent of ὑποζώματα, and we know from Isidore that these ropes were differently located from the *mitra* and used for a different purpose. The ropes with which the ram in Athenaeus *De Machinis* was girt (ὑποζώννυται) ran lengthwise with the beam, not around it, as is verified by the corresponding passage in Vitruvius. The good ship *Argo* could not have been so girded amidships unless more was done than is in fact told us. How the sailors in the storm described in *Acts* could have made any effective use of the *βοηθείαις* seems incomprehensible if the cable merely encircled the hull. And finally there is the very convincing argument of Mr. Schauroth that the ὑποζώματα were not ropes wrapped around the hull.

Mr. Schauroth's own theory¹ is that the ὑποζώματα "were under-girders of rope, or perhaps chains, transversally stretched across the ship's hold under the deck, and attached at either end to one of the stout rib pieces. If two or more ropes were thus attached in one place, it would be possible, by means of a bar or lever inserted between them, to twist them as taut as desired, and so brace up the ship for an emergency."

There are, however, three serious difficulties to this theory:

(1) There is no positive evidence that such a device was ever employed on ancient ships.

(2) It is not in accordance with the evidence we possess. Mr. Schauroth himself admits that the evidence of the Latin writers shows that these girders ran from stem to stern, and he advances no sufficient reason why their evidence should be excluded. It also seems to me inconsistent with the evidence from Athenaeus and *Acts*. The evidence from Athenaeus indicates that the ὑποζώματα were longitudinal trusses, not cross-braces. In the account in *Acts*, on Mr. Schauroth's theory, the statement *βοηθείαις ἐχρῶντο* would appear to have no real meaning. Why should supports be used for the cross-braces he suggests?

(3) There does not seem any effective use that such ropes could serve, which could not be better served by the parts in use. To be

¹ *Harvard Studies*, XXII, p. 173.

effective, such ropes must be strongly fastened to the ribs; and if the ancient shipbuilders could have designed such fastenings for ropes, it would seem that the deck-beams, or, in the case of open boats, the thwarts, could have been just as effectively fastened. A beam of wood will stand a pull just as well as a rope and it would have distinctive advantages. The inward thrust on the ribs is quite as important to guard against as any outward push, and the deck-beams or thwarts would resist this thrust, which the ropes would not. Ropes are shortened by dampness and stretch again when dry. No rope-brace, therefore, would remain effective without more or less constant attention. Wood is not so altered lengthwise by dampness or dryness, and wooden beams, therefore, would save trouble. The rope-braces would have to be located at some distance below the decks to permit of being twisted, and they certainly would have been much in the way of the crew. Indeed it seems very difficult to see how they could have been reached after the ship was loaded.

Mr. Schauroth instances the use of his device on canal-boats; but canal-boats are not usually sea-going ships and the strains to be met are different. Still, I think it cannot be asserted that his device might not have been occasionally useful on ancient ships. His theory, therefore, cannot be dismissed on the ground of inutility; but such considerations in connection with the other evidence, seem to me to render it improbable that the *ὑποζώματα* were inside cross-braces.

While Chatterton, in his recent work on sailing ships,¹ does not discuss the *ὑποζώματα* at length, the following citations would seem to indicate that he thought them the same as the cables used on the Egyptian ships.

When a ship is sailing on the sea, she is thrown up by the motion of the waters till she rests pivoted on the crest of a wave. The middle of the ship is thus supported, but the bow and stern, not being water-borne, have a tendency to droop while the centre of the ship tends to bulge up. This is technically known among naval architects as "hogging." In the case of ships with an enormous overhang, unsupported by water such as was the case of the Egyptian ships and is now the fashion with our modern yachts, this hogging would need to be guarded against. . . . In the *Hatshopsitu* ships we see the "hogging" strain guarded against by a powerful truss of thick rope.

¹ E. K. Chatterton, *Sailing Ships, The Story of their Development from the Earliest Times to the Present Day*, London, 1900, pp. 56 ff., 65.

This truss leads from forward, sometimes being bound round — undergirding — the prow: sometimes it is made fast inside, perhaps to the deck or to the floors. It then leads aft, being stretched on forked posts until it reaches the mast, where it is wound round in a sort of clove-hitch, and then continues aft again being stretched on other forked posts until it is finally girded round the counter. This truss was as large as a man's waist, and has been calculated by Commander T. M. Barber of the United States Navy to have been able to withstand a strain of over 300 tons. . . . Like the Egyptian ships, these ancient vessels were also provided with a stout cable — the ὑπόζωμα in Greek, *tormentum* in Latin.

The results of this discussion may, I think, be summed up as follows. We know from the Egyptian monuments that the preclassical navigators used a rope-truss to keep their ships from hogging. We know that the Greek and Roman sailors used rope-cables, called in Greek ὑποζώματα and in Latin *tormenta*, to strengthen their vessels, and especially to enable them to stand stormy weather. From the references to them in Athenaeus and Vitruvius, these cables probably ran from stem to stern and were used as trusses. From the account in the *Argonautica* it appears that they ran inside the boat and not on the outside. From the account in *Acts* it appears that "supports" were used with them, a term which neatly applies to the device depicted by the Egyptians, but seems to have no real meaning on either of the other theories. Finally, we have Isidore's definitions, one of which, that of *tormentum*, well fits this Egyptian rope-truss; and the other, that of *mitra*, might fit the ropes encircling the overhangs fore and aft which we see in the Egyptian pictures. The Egyptian monuments prove that the rope-truss was in use for more than fifteen hundred years. We know now something of the intimate connection of Crete with Egypt, of the extensive commerce of the former and of its influence on Greece. It is certainly probable that this ancient device passed on to the Greeks and from them to the Romans. The form of the device fits the evidence found in the Greek and Roman writers. On the whole, therefore, it seems probable that the ὑποζώματα were nothing but the ancient rope-truss which we see pictured by the Egyptian artists.

A NEW APPROACH TO THE TEXT OF PLINY'S LETTERS

BY EDWARD KENNARD RAND

ARTICLE I

THE ancient uncial fragment of Pliny's *Letters* now in the Pierpont Morgan Library in New York gives us, if I am right, a surer means of approach to the text of the *Letters* than we have had before. In the joint publication which my friend Dr. E. A. Lowe and I prepared for the Carnegie Institution of Washington,¹ I endeavored to substantiate my views, which include a more favorable estimate of Aldus than he has received at the hands of recent editors. Naturally, if my theory is sound that we have in the Morgan Fragment a part of that venerable *Codex Parisinus* which Aldus praises to the skies in his edition of 1508, and if it is further true, as seemed to me probable, that he collated the *Parisinus* minutely and clung tenaciously to its readings, then the value of his edition appreciably mounts; it is only one remove from a veritably ancient text, three or four hundred years older than the oldest manuscript thus far brought to light. More important, further, than the exact extent of this superiority in age is the fact that the Morgan Fragment was written in antiquity before the Dark Ages which preceded the period of Mediaeval copying set in. Nobody would maintain that Aldus printed an exact reproduction of his ancient book. The *Parisinus*, like any manuscript, doubtless contained errors, which Aldus as an editor would hardly accept. He had other manuscripts at his disposal as well as the contemporary editions. He may sometimes have been unwise in deserting his chief source for more recent authorities; he may have occasionally resorted to emendation or adopted the conjectures of other scholars; his text also contains, as is inevitable, a certain number of printer's errors. But the point is this. If the Morgan Fragment is part of the *Parisinus* and if

¹ *A Sixth-Century Fragment of the Letters of Pliny the Younger. A study of six leaves of an uncial manuscript preserved in the Pierpont Morgan Library in New York*, by E. A. Lowe and E. K. Rand, 1922, pp. 37 ff.

Aldus followed that manuscript as closely in the rest of the *Letters* as he does in this part, then we should give his readings throughout a more respectful consideration than has thus far been accorded to them. To what extent our present texts of Pliny will be affected thereby can be determined only when we have put Aldus to this new test. I intend to apply it, so soon as other duties permit me. It is a long and elaborate process; the object of the present paper is to prepare the way.

The arguments which I set forth in favor of the hypothesis that the Morgan Fragment (II) is part of the *Parisinus* (*P*) have been either approved or not rejected by certain scholars of eminence,¹ but my presentation of the case has found no favor, I am sorry to say, with my friend Professor E. T. Merrill,² the latest editor of the *Letters*,³ whose knowledge of the details of the text and of its history is surpassed by none. As I think that Mr. Merrill has not accurately described the nature of my argument, I will repeat and clarify, if I can, its essential points. Incidentally I will reply to the many objections which Mr. Merrill has raised to my mode of reasoning. I will then proceed to a fresh examination of the Bodleian volume once possessed by Budaeus, which according to Mr. Merrill is our most immediate authority for the readings of *P*.

I. THE MORGAN FRAGMENT AND THE *CODEx PARISINUS* OF ALDUS

The arguments which led me, after considerable study, to the conclusion that the Morgan Fragment may be most probably regarded

¹ H. Omont in *Bibliothèque de l'École des Chartes*, 1922, pp. 474 ff.; P. Fabia in *Journal des Savants*, 1923, pp. 87 ff.; A. Klotz in *Philologische Wochenschrift*, 1923, coll. 509 ff. and *Literarisches Zentralblatt*, 1923, p. 418; C. Wendel in *Zentralblatt für Bibliothekswesen*, 1923, pp. 159 ff. See also the review (*Philol. Woch.*, 1923, coll. 865 ff.) by F. Levy of the important work of G. Carlsson, *Zur Textkritik der Plinius-briefe*, Lund, 1922. This publication, sent to me by the courtesy of the author, arrived after the present paper was in type. The attempt of this careful scholar to return to Keil and exalt the Nine-book family of manuscripts (*MV*) above the Ten-book family (*BFa*) will doubtless convince others besides Levy. At the present moment, I am not altogether won over; I will return to this question in my later articles. It is only of incidental concern in the present paper, which deals with Aldus's use of his *Parisinus*, whatever the nature of the text of that codex.

² "The Morgan Fragment of Pliny's *Letters*," in *Classical Philology*, XVIII (1923), pp. 97 ff.

³ Leipzig, Teubner, 1922.

as once a part of the lost *Parisinus* of Aldus, start, as is natural, with Aldus's account of his ancient book.

A. *Codex venerandae antiquitatis*

1. *Aldus's description of his codex*

Aldus states in the preface to his edition of the *Letters* in 1508 that he had used for his text an ancient manuscript of Pliny brought down from Paris by the Venetian ambassador Mocenigo. He had previously received from Fra Giocondo of Verona a careful copy of this manuscript, along with six other volumes, of which some were manuscripts and some were printed books, collated by Giocondo *cum antiquis exemplaribus*. But now the ancient original itself had come. Aldus calls it a book *venerandae antiquitatis*, so ancient, in fact, that it might have been written in the days of Pliny himself. He describes its sovereign importance for the text in correcting Greek words, in replacing false readings by true and in filling important gaps; Aldus was the first to print the complete text of all ten books as we have it to-day. So elated is he by the discovery of such a manuscript that he even thinks it possible that the lost decades of Livy may now come to light.

This enthusiasm of Aldus impressed me as sincere; it seemed to have the ring of truth, as though his "ancient" manuscript might really have descended from antiquity. Mr. Merrill, on the contrary, thinks that some persons have been misled "by the forensic expressions of praise lavished by Aldus upon the manuscript brought him by his exalted patron, Aloisio Mocenigo."¹ Whether Mr. Merrill still thinks that the *Parisinus* might have been, among various possibilities, "in a Gothic hand of the twelfth or thirteenth centuries,"² I do not know, but he obviously would not credit Aldus's words at their face value. Some discount is appropriate in any panegyric; supposing that the Morgan Fragment was once a part of *Parisinus*, we must deny that it was written in the time of Pliny. An error of this sort, however, would not preclude the possibility that *P* was genuinely antique. There is contemporary testimony of some importance to the sincerity

¹ "On the Use by Aldus of his Manuscripts of Pliny's *Letters*," in *C. P.*, XIV (1919), p. 29.

² *C. P.*, II (1907), p. 134.

of Aldus and the value of the ancient source at his command. Erasmus was a member of the household of Aldus not long before the edition of Pliny saw the light. He had arrived in Venice early in 1508 for the purpose of seeing through the Aldine press the second edition of his *Adagia*.¹ Aldus had evidently made him acquainted with the nature of the new edition and of the manuscript to which it owed so much, for in the course of an enthusiastic digression on the merits of Aldus,² Erasmus declares that if the latter can carry out his plans, the best authors will appear in renovated texts. He adds:

Quod simul atque contigerit, tum vero palam fiet, quantum adhuc bonorum codicum in abdito sit, vel retrusum ob negligentiam vel suppressum quorundam ambitione, quibus hoc unum cordi est, ut soli sapere videantur. Tum denique cognitum erit, quam prodigiosis mendis scateant autores, etiam hi qui nunc satis emendati putantur. Cuius rei si cui libebit, uelut ex gustatione, coniecturam facere, Plinianas Epistolas, quae propediem ex Aldina officina prodibunt in lucem, cum uulgatis exemplaribus conferat, quodque ibi deprehenderit, idem in aliis expectet autoribus.

The new text of Pliny is for Erasmus typical of the entire undertaking of Aldus. He means, I should infer, not that Aldus is merely a skilful emender of texts, but rather that he has resorted to good sources — *boni codices* — of which he thinks others must be in hiding. Erasmus would hardly have spoken thus if he had known that Aldus's forthcoming edition of Pliny owed its improvements to some Gothic book of the twelfth or the thirteenth century, helped out with the editor's conjectures.

In attempting to justify Aldus's description of his *codex Parisinus* as an ancient book, I suggested that in calling the script different from that of his own day — *diversis a nostris characteribus* — he might have had in mind the general distinction between minuscule script and majuscule and used the term *nostris characteres* to indicate the former variety. This I called a "position palæographically defensible," for the reason that the Humanistic bookhand, with which Aldus was familiar, represents a revival of the essential traits of Carolingian script. "Our script," to Aldus, might thus be that which was devel-

¹ A delightful account of Erasmus's visit at the Aldine establishment is given by P. de Nolhac, *Érasme en Italie*, 1888, pp. 31 ff.

² *Adagia*, at the beginning of *Chiliadis Secundae Centuria* I, Leyden edition, 1703, II, p. 402e.

oped after the period of antiquity was over; a book written *diversis a nostris characteribus* would, on this definition, be genuinely antique, at least in the opinion of the person who so designated it. Hereby, according to Mr. Merrill, I almost put the reader out of humor by a "fantastic elaboration."¹ He expresses surprise, or whatever an exclamation-point expresses, at the idea that "Aldus may well have known, and meant to say, that *P* was written in what we should call a majuscule hand and therefore to be dated before, say, the era of Charlemagne," and adds that "this must be quite a new suggestion of the precision of knowledge of the history of palæography possessed by a Renaissance scholar, and one who yet (according to Mr. Rand) believed uncial to be the mode of writing in Pliny's time."

I will admit at once, that my interpretation of "*nostris characteres*" is what I called "charitable," and withdraw my suggestion that by this term Aldus had in mind the entire course of minuscule writing, but the essential point in my contention I still maintain, which is that Aldus might have recognized a majuscule hand in *P*, if the script was that of *II*, and that for this reason he regarded his book as a genuine product of the time of the ancient Roman Empire.

2. Notions of palæography in the Renaissance

The modern science of palæography dates from the time of Jean Mabillon and his monumental *De Re Diplomatica* of the year 1681, but many attempts had been made before his day to classify the different forms of script.² I venture to think that a scholar of the time of Aldus was aware that the lettering employed in an ancient capital or uncial codex was of a different species from that with which he was familiar. Ancient inscriptions were abundantly accessible; they might have suggested to a person with eyes that if an entire manuscript was written in such a style, its date might not be far removed from the period of the inscriptions. In fact, this very observation is made by the eminent Virgilian scholar Giovanni Pierio Valeriano, who speaks

¹ *C. P.*, XVIII (1923), p. 108. From now on, unless otherwise stated, page references are to this article.

² Traube had planned as a work for himself or some pupil a "*Geschichte der Paläographie vor Mabillon*"; see Lehmann's note in the *Vorlesungen und Abhandlungen*, I (1909), p. 25. This history still calls for a chronicler.

thus in his *Castigationes et Varietates Virgilianae Lectionis*, published in Rome in 1521:¹

Antea quam rem ipsam aggrediamur, nomina, quibus insigniores quosdam codices citamus, praedocere visum est. Ea sunt: Codex Romanus, ille quidem dubio procul antiquissimus. Eum vero ideo Romanum appellamus, quod eius characteres Romanis propiores sunt, iis quippe, quos in antiquis marmorum, aut ex aere tabularum inscriptionibus, et in nummis, saeculis illis elegantioribus notatos ubique legimus. Custoditur is in interioribus Vaticanae bibliothecae penetralibus magna diligentia digitalibus pene litteris perscriptus.

Valeriano, or Pierius, as he is also called, then describes some of his inferior manuscripts, of which two seem to him of most value:

Alter, qui minoribus est litteris et ipse admodum vetus, a paginarum facie Oblongus nuncupabitur. Est et Longobardicus, quem non omnino pigeat evoluisse.

He mentions a number of others, and naturally includes the Medicean *inter emendatos*. We see a clear enough distinction here made between majuscule and minuscule script. The latter is designated as *minores litterae*. The former is certainly recognized as both large and antique; Pierius virtually calls the script "Roman," and for this reason christens the book with the same name. The *Oblongus* was a minuscule book, and yet *admodum vetus*. What the *Longobardicus* was is matter for debate. Perhaps it was *Vaticanus Latinus* 1573 s. xi, which is in a Beneventan hand, but it may just as well have been a book in some other script, which whether early or late in date seemed wild and primitive in style;² my guess, with which Dr. Lowe would apparently agree, is that to Pierius and to the scholars of his time and before, Lombardic script was that in which the cursive element was prominent.³

¹ Ed. R. Stephanus, Paris, 1529, p. 7.

² Listed by Lowe, *The Beneventan Script*, 1914, p. 362. Ribbeck, *Prolegomena Critica ad P. Vergili Maronis Opera Maiora*, 1866, pp. 354 ff., gives enough readings from the *Oblongus* and the *Longobardicus* to show that their texts are inferior. Pierius's citations are so numerous that it ought to be possible to identify these manuscripts.

³ The history of the term *scriptura* or *littera Beneventana* has been written by Lowe (*op. cit.* pp. 22 ff.), and after his and Traube's arguments (*Vorlesungen und Abhandlungen*, I, pp. 25 ff.), there is no doubt of its antiquity or its appropriateness

As Pierius is a younger contemporary of Aldus, perhaps some will argue that the amount of palæographical discrimination displayed by him was a novelty unknown to Aldus, who died before Pierius's book on Virgil saw the light. But an equal interest, and an equal competence, in matters palæographical was shown by Aldus's friend and earlier contemporary Politian.¹ Of the famous Bembine Terence, a book in rustic capitals, which Politian collated in 1491, he declares with vigor in a note inscribed in the volume itself:²

Ego Angelus Politianus, homo vetustatis minime incuriosus nullum aequè me vidisse ad hanc diem codicum antiquum fateor.

as a description of what since Mabillon's time has erroneously been called "Lombardic." But it is just as clear that "*littera Longobardica*" also meant something. It too was a technical term. For instance, the catalogue of the Bobbio books compiled in 1466 lists a *Iosephus antiquissimus cum Aegesyppo in littera que dicitur longubarda* (Sabbadini, *Le Scoperte dei Codici Latini e Greci ne' Secoli XIV e XV*, 1905, p. 138); *que dicitur* is significant. Mabillon, knowing the vogue of the term, sent to Italy to find out what it did mean (Lowe, p. 25). Dr. Lowe cites a catalogue as early as the year 1336 (p. 38) in which the script of almost every book is labelled. Of the terms used, *littera beneventana*, according to Dr. Lowe, means what it does to-day and *littera gallica* is the ordinary type of minuscule. *Littera langobarda*, he thinks, may likewise mean Beneventan, though it may perhaps refer to some specially cursive hand or to a foreign variety like Insular or Visigothic. Now when we note that Fulvio Orsini in the catalogue of his books compiled in 1582 designated the scholia in the Bembine Terence as "in lettera longobarda" (P. De Nolhac, *La Bibliothèque de Fulvio Orsini*, 1887, p. 358), we find, I think, a clue to the use of the term in general. It is applied, I take it, to any book in which cursive traits are specially noticeable. A manuscript described by Politian as *vetustissimus liber literis langobardicis* (Sabbadini, *Scoperte* etc., 1905, p. 151) is the Ambrosian Columella (L. 85 *sup.* s. IX-X). This manuscript, which is carefully treated by A. W. Van Buren (*Supplementary Papers of the American School of Classical Studies in Rome*, I (1905), pp. 157 ff., with a good facsimile, Pl. XVIII) is, as Traube declared, in the sort of Insular and specifically English script cultivated at Fulda in the ninth century; the neat variety of the Insular round hand retains plenty of cursive traits. When the term Lombardic is also applied to Beneventan, that, too, is an appropriate use of it; for ancient cursive features in large numbers are conserved and embellished in the beautiful Beneventan hand. Beneventan, from this point of view, is a special kind of "Lombardic." It was the particular kind that, unfortunately for the history of palæography, was sent as a typical specimen to Mabillon.

¹ The dates of Politian are 1454-1494, of Aldus 1449-1515 and of Pierius 1477-1558.

² De Nolhac, *op. cit.*, p. 238; *P. Terenti Comoediae*, ed. F. Umpfenbach, 1870, pp. vi ff.

And again:¹

Ego Angelus Politianus contuleram codicem hunc Terentianum cum venerandae vetustatis codice maioribus conscripto litteris, quem mihi utendum commodavit Petrus Bembo, Venetus patricius, Bernardi iuris-consulti et equitis filius, studiosus litterarum adulescens. Observavi autem, quod consuevi, ut ad unguem exscriberem, etiam quae plane mendosa videbantur.

The illustrious possessor of the book, Cardinal Bembo, apparently appreciated the value of his treasure. He inserts a note of ownership as follows: *Est mihi Bernardi Bembi qui post eius obitum maneat in suos antiquissimae antiquitatis reliquiae*. In another note he exclaims with fervor: *Codex mihi carior auro*, and in another his feelings find expression in a verse of Boethius: *O foelix nimium prior aetas*.² Politian also observes that the script resembles that of two other well-known books: *Erat enim liber paene litteris simillimis earum quibus et Pisanae Pandectae et Vergilianus Palatinus codex est exaratus*.³ Two of these books, the Terence and the Virgil,⁴ would to-day be assigned to the same category of script, the rustic capital. The third, the famous *Digestorum Codex*, now in the Laurentian Library in Florence, written in a late variety of uncials with some cursive traces especially at the ends of lines, is generally dated s. VI-VII, that is, just at the end of antiquity or just at the beginning of the Dark Ages.⁵ Politian did not distinguish, as we do to-day, between capitals and uncials,⁶ but

¹ *Codices e Vaticanis selecti phototypice expressi opera Curatorum Bibliothecae Vaticanae*, I (1899), p. vi.

² See p. 85, n. 2.

³ From a note inserted by Politian in *Laur. Med.* XIV, 38, a manuscript of Terence. See *Cod. e Vat. Sel.*, II, p. vi.

⁴ This is of course not the manuscript to-day called *Palatinus* (*Vat. Lat.* 1631) but the *Romanus*, as Pierius later named it (*Vat. Lat.* 3867).

⁵ See E. M. Thompson, *Introduction to Greek and Latin Palaeography*, 1912, p. 304, Facs. 97. Politian puts the book too early in the sixth century, for he elsewhere calls it the archetype of Justinian's work (*Liber Miscellaneorum* 77): *Quin in Pandectis quoque iis, quae nunc Florentiae publice asservantur, libro ipso Iustiniani principis archetypo, non aliter quam per e notatur id nomen*. This is from Politian's famous note on the proper spelling of Virgil's name (in Latin), a note which shows that he had studied both inscriptions and various sorts of manuscripts for evidence on the point.

⁶ De Nolhac, *op. cit.*, p. 85.

he at any rate was aware of the character and the antiquity of majuscules. In fact, that is the very term he uses for the *Romanus* of Virgil — *ille codex antiquissimus Vergilianus qui istic in intima Palatina bibliotheca adservatur maiusculis characteribus exaratus*.¹ He speaks of a Manilius in such terms that according to Sabbadini he had before him a book "in maiuscolo o comunque anteriore al sec. IX."²

If Politian recognized majuscule script well enough to call it by that name, he doubtless could tell minuscule when he saw it, whatever the term by which he referred to it. It is clear that some of the books which he calls *vetusti*, *antiquissimi* and the like, are in minuscule, as we can see them with our eyes to-day.³ Whatever inaccuracies he committed in the dating of minuscule manuscripts, he would not, I venture to believe, put them in the same period as the ancient majuscules which he describes. And he can make distinctions among the later books. In discussing a passage in Martial,⁴ he refers to one manuscript written *vetustissimis langobardis literis*, one which he calls *semiveterem* and one which is *mediae antiquitatis*. Not very long after Aldus and still within the life-time of Pierius, Petrus Crabbe, in editing *Acta Conciliorum*, 1538, dated an English codex, quite in the style of Mabilon, as *vetustum quoddam opus annorum videlicet sexcentorum*.⁵

The term "majuscule," though used by Politian, may not have been invented by him. It seems more likely that it proceeded from the authority of the Vatican, for in the catalogue of 1475,⁶ the *Romanus* of Virgil is described as *Virgilius, in maiusculis, ex memb(rano) in albo sive rubeo reinquinternato* and in that of 1481 as *Virgilius antiquus, litteris maiusculis scriptus ex memb(rano) in rubeo*. Politian did not see the book, it would seem, until his visit in Rome of 1484.⁷ The brief phrase "*in maiusculis*" in the earlier catalogue sounds technical, as though the term had had something of a history before that time. The *Romanus* came to the Vatican between 1455 and 1475.⁸

Towards the end of the fifteenth century, we have a document of palæographical importance in the catalogue of the library of Fulvio

¹ *Epist.* 4, 9; Sabbadini, *Scoperte*, etc., 1905, p. 154.

² *Ibid.*, p. 170.

³ *Ibid.*, p. 152.

⁴ *Lib. Misc.* 23.

⁵ See Lehmann in Traube's *Vorlesungen und Abhandlungen*, I, p. 28.

⁶ *Cod. e Vat. Sel.*, II, p. v.

⁸ *Cod. e Vat. Sel.*, II, p. v.

⁷ Sabbadini, *Scoperte*, etc., pp. 153 ff.

Orsini which that scholarly nobleman himself compiled about the year 1582.¹ Among his treasures were various books once owned by Cardinal Bembo, including the Terence, the illustrated Virgil (*Vaticanus Latinus* 3225), which came to Bembo from Pontano, and the Vatican Fragment of Virgil (3256) once the possession of Claude Dupuy. All of these are described by Orsini as “di lettere maiuscole”; the ancient term “majuscule” is still in use. De Nolz has no doubt that the learned owner of these books regarded them as the products of “l’époque classique.”² Of his use of “lettera longobarda” I have already spoken. Further, he attempts with some elaboration to date his minuscule manuscripts, designating their ages as “di 400 anni,” “di 600 anni,” “di più di 700 anni” and so forth, up to “mille anni.”³ According to De Nolz,⁴ he applies “antico” to a manuscript written before the fifteenth century and “antichissimo” to one before the fourteenth. Some of his datings are considerably off the track;⁵ in general, he pushes the period of minuscule writing too far back into the past, regarding its earliest appearance, it would seem, as occurring in the sixth century.⁶ Nevertheless, though his classes are mal-adjusted to the centuries, he has devoted no little attention to the determination of these classes, and has grouped in the same category books that we know are not far separated in point of time. He can also compare a manuscript of his collection with one outside it.⁷ This catalogue, with the notice of Orsini’s library by Angelo Rocca in 1591,⁸ the Vatican index begun by Rainaldo in 1594 and the previous Vatican catalogues are a few of the sources to which the future investigator of the history of palæography in the Renaissance should direct his attention.⁹

¹ De Nolz, *op. cit.*, pp. 116 ff., 333 ff.

² *Ibid.*, p. 276.

³ *Ibid.*, pp. 358 ff.

⁴ *Ibid.*, p. 275.

⁵ *Ibid.*, pp. 275 ff.

⁶ Some of the books called “di mille anni” are really of the ninth and even of the tenth century, as for instance the *Consolatio Philosophiae* of Boethius, *Vat. Lat.* 3363 s. IX (not s. X, as De Nolz has it). In this case, as De Nolz (p. 276) remarks, Orsini may have been misled by the use of majuscules in the poems of the *Consolatio*.

⁷ *Vat. Lat.* 3314 s. IX, Porphyrio, with *Vat. Lat.* 3868 s. X, Terence. *Ibid.*, p. 226.

⁸ *Ibid.*, p. 109.

⁹ On Rainaldo see *Cod. e Vat. Sel.*, II, p. vi. His descriptions are detailed and systematic, as one specimen will show: 3325. *Virgilius, litteris maiusculis cum figuris . . . Ex perg(ameno) c(hartae) s(criptae) 75, ant(iquus) vet(us) in quarto*.

In fine, the humanists of the sixteenth century, and the last part of the fifteenth, while making numerous errors in their judgments of individual books, had a fairly intelligent opinion, it seems to me, of the general development of script. They had a feeling for the different periods in this development, though they shoved this whole system too far into the past. Just as Orsini assigned to his earliest minuscules too ancient a date, so the majuscule books were generally invested with too hoary an antiquity. According to Rainaldo, the *Romanus* of Virgil was written under Constantine.¹ But progress in accuracy was made. If Bembo, who died in 1547, thought that the Bembine Terence had descended from the age of Cicero,² Angelo Rocca in 1591 brought it down to the reign of Alexander Severus.³

3. *Aldus's notions of palæography*

(a) *Aldus and Politian*

But now let us come a bit nearer to Aldus. He was twenty-six years old at the time when a Vatican librarian was using "majuscule" as a technical term. Possibly he might have heard of it by that time. He had indubitably heard of it in 1498, for in that year he published the works of Politian. I am inferring that Aldus likewise read them. On his painstaking methods as a reader of proof, we have the word of Erasmus, who, as we have seen, saw the second edition of his *Adagia* through the Aldine press. A hired proof-reader, named Seraphinus, corrected the first impressions, then Erasmus made any changes that he wished, and finally Aldus himself went through the pages. When Erasmus, evidently surprised at such scrupulousness, asked Aldus why he took so much trouble on himself, the latter replied with a genial courtesy, "I am learning something by the way."⁴ I do not

¹ See note 9, p. 88.

² This opinion of Bembo's is reported by his friend, Ludovico Beccadelli, Archbishop of Ragusa. See De Nolz, "Le Virgile du Vatican et ses Peintures," in *Notices et Extraits des Manuscrits*, XXXV (1897), p. 104.

³ De Nolz, *Fulvio Orsini*, p. 110.

⁴ "Aldus saepenumero praedicabat se admirari, qui tantum scriberem ex tempore, idque inter tumultus circumstrepentium. Operis mei formas extremas castigavi, tantum in hoc si quid vellem mutare. Nam opus alioqui suum habebat castigatorem conductum nomine Seraphinum. Aldus post me legit; cum rogarem cur hoc laboris caperet, interim, inquit, studeo." *Opera Erasmi*, Leyden, 1706, IX, p. 1137. Cf. III, p. 1751.

know whether Aldus treated all of his publications thus conscientiously, though it would not be surprising if he did, but I am tolerably sure that he must have at least read a work accepted for his press, especially if the author was his greatly venerated friend Politian. Let me quote from Aldus's preface to his *Omnia Opera Angeli Politiani*. With his usual desire to have the text just right, he warns his friend Marinus Sannutius that there may be some errors in it:

Scito non esse haec edita ab ipso sed ab amicis, et praecipue ab Alexandro Sartio Bononiensi, literatis omnibus pergrato viro, qui amicissimi viri quaecumque habere potuit opera multum ac diu et accurate quaesita imprimenda curavit.

This clearly is a reference to the Florence edition of 1489. We can also see why Politian's presentation copy to Alexander Sartius, which Harvard College Library owns, was especially appropriate. Aldus then proceeds:

Est igitur dignissimus venia Politianus noster, si quid in eius scriptisprehendetur vitii, quandoquidem emendaturus, si licuisset, erat, immo si annos suos vixisset et leges ex pandectis quae olim pisanæ fuerunt, in pristinam, quod coeperat facere, lectionem restituisset, commentariosque in illas, quod praedicabat, non barbare, sed more romano, et doctissime confecisset.

As Aldus is familiar with Politian's labors on the text of the *Pandects*, I should suppose that he had read what Politian says of the Florentine manuscript, or had heard him talk about it; the words here used imply that the codex had a certain antiquity. If he had read all that Politian says about manuscripts in this edition of 1498, Aldus, *si interim studebat*, would have learned a good deal about majuscules. I will add a passage to those already cited. A letter from Petrus Crinitus to Alexander Sartius is included among the *Epistulae* of Politian, as it contains an epigram in his honor.¹ In this letter Crinitus gives proof, necessary at that time, that Terence did not write the arguments of his plays. The writer remarks:

In vetustissimo itaque Terentii codice literis ut nostrae pandectae maiusculis ad hunc modum super Terentianis argumentis G. SULPICI APOLLINARIS PERIOCHA, cuius rei testis mihi et Petrus Bembus mira iuvenis et indole et ingenio.

Here we see "majuscules" not only named but graphically portrayed. Might Aldus have seen the Bembine Terence with his own eyes?

¹ 12, 22.

Bembo was a member of Aldus's Academy and deeply interested in his work.¹ Aldus had examined the autograph manuscripts of Petrarch in Bembo's library before publishing the *Cose Volgari* of the poet in 1501; Bembo allowed him to print his edition straight from them.² He hardly could have been so unkind as to deny Aldus a glimpse of his Terence and of some of his other ancient treasures. At any rate, Aldus sides with Crinitus on the arguments of Terence's plays, for in the document, written in 1503, in which he warns the public against the counterfeit editions of Lyons, he points out as one of their defects, the false attribution of the *argumenta* to Terence,³ whereas their real author, he states was Sulpicius Apollinaris: *Sic enim in vetustissimis habentur codicibus, C. Sulpitii Apollinaris periocha.*

(b) Aldus and the script of Petrarch

Aldus's intelligent interest in script needs no demonstration. He had studied script before devising his epoch-making font of type, first exhibited in the *Virgil* of 1501.⁴ He states, apparently, that he had formed it on the hand of Petrarch,⁵ and though it is the custom at present to question such a statement, since the Aldine font is obviously nothing but a variety of humanistic cursive, a style very different from Petrarch's hand,⁶ I somehow feel that Aldus's words deserve a new and more respectful examination.

Aldus and others described his type, in exact and technical terms, as *lettera corsiva* or *cancellaresca*.⁷ He aimed at the effect of ordinary hand-writing, with its ligatures — another technical term that he

¹ On Aldus's Academy, see A. Firmin-Didot, *Alde Manuce et l'Hellénisme à Venise*, 1875, pp. 147 ff.

² See below, pp. 92 ff.

³ See H. Omont, *Catalogues des Livres Grecs et Latins imprimés par Alde Manuce à Venise*, 1892, Plate (II); Firmin-Didot, *op. cit.*, pp. 240 ff.

⁴ Firmin-Didot, *op. cit.*, pp. 155 ff.

⁵ *Ibid.*, p. 164.

⁶ As shown, for instance, in F. Steffens, *Lat. Paläog.*², Pl. 101, which gives a page of Petrarch's *Virgil* with marginalia in his own hand. For a beautiful specimen of humanistic cursive, see the Lucan written by Pomponius Laetus; Ehrle and Liebaert, *Specimina Codicum Latinorum*, Pl. 50.

⁷ Firmin-Didot, *op. cit.*, pp. 159, 189, 226 ff. For three specimens of the official Papal script, dated 1472, 1512 and 1606, see Steffens, *op. cit.*, Pl. 116. The style of that of 1512 (Julius II) is nearest to that of the Aldine font.

employed.¹ He is an expert in the minutiae of letter-forms, and he knows just what he has contrived. He can tell, and he wants the reader to know, just how his type differs from the imitative form in the pirated editions of Lyons.² He has not only an eye but a nose for details. He declares that the very paper used is *deterior*, and *nescio quid graue olens*. As for the forms of the letters,

Characteres uero diligentius intuenti sapiunt (ut sic dixerim) gallicitatem quandam. Grandiusculae item sunt perquam deformes. Adde quod uocalibus consonantes non connectuntur, sed separatae sunt. In nostris plebrasque omnes inuicem connexas: manumque mentientes: operae pretium est uidere.

In other words, the imitations have missed the very point. The type is delightfully cursive and informal, befitting the small size of the volumes, which you slipped into your pocket, or saddle-bag, for a saunter in the country and an hour with Horace *sub platano*. Of course Aldus had a lively financial concern in the success of his publications,³ and would know his own font if anything. But the eye which detected "a certain Frenchness" in the counterfeit type and noted that it had no ligatures, could hardly have failed to appreciate that the treatment of ligatures in the modified Gothic hand of Petrarch differs from that in the humanistic cursive and in his own type.

Now there is no doubt that Aldus was shown an autograph manuscript of Petrarch by Bembo, which the latter had borrowed from a Paduan gentleman and which he owned later himself.⁴ Petrarch's hand, moreover, should not be studied from only one specimen of his writing. He practised various kinds of script, which in Protean fashion

¹ See his protest to the Venetian Senate against the counterfeits of his invention; Firmin-Didot, *op. cit.*, p. 227: *perche ha facto lettere greche cum ligature che pareno cum calamo, et ha ritrovato invention et inzegni che ciascuno se ne maraveglia, et piu di novo ha excogitato lettere cancellaresche sive corsive latine bellissime che pareno scripte a mano.*

² See Omont, *loc. cit.*

³ A somewhat too commercial view of Aldus is presented in Mr. D. B. Updike's fine book, *Printing Types, their History, Forms and Use*, 1922, I, p. 128, who remarks: "If Aldus had been named 'Brown,' Raibolini 'Smith' and Petrarch 'Jones,' the venture would appear to us more what it appeared to the Venetians of that day." Somehow the equation "Aldus = Brown, Petrarch = Jones" will not work out. On the meaning of Aldus's achievement see below pp. 137 ff.

⁴ See below pp. 147 ff.

he could change at his caprice from moment to moment.¹ As one of these varieties is cursive,² one might imagine that Aldus noted a page of this sort and modeled his type on that. But this can hardly have been the case. Petrarch's cursive is not of the kind that Aldus or anybody else would care to immortalize in type; it is the sort of scribbling that is employed in a first draft, or when words come too quickly for calligraphy. Even Petrarch would not have relished fame at such a price. Furthermore, though it is a cursive hand, it differs vastly from the humanistic cursive. The manuscript of most importance to Aldus, and one which he surely saw, is in Petrarch's careful book-hand.³ That would have been his most natural model, if he had the idea of perpetuating Petrarch's script in a font of type. But this book-hand, once more, is neither humanistic nor cursive.

Moreover, if the imitation of Petrarch's hand is really a feature of the Aldine type, it is remarkable that, to the best of my knowledge, Aldus nowhere mentions this feature in his later descriptions of his type. Nor is Petrarch mentioned by Bembo, who in the edition of Cato and other ancient authorities on farming, 1514, confirms the privilege granted to the edition by Pope Leo X and has occasion to speak of the beauties of the type.⁴ No knowledge of the similarity of the Aldine type to Petrarch's hand is implied by Lorenzo of Pavia, a friend of Aldus, who was buying Aldines and other works of art for Isabella Gonzaga, Marchioness of Mantua, and who arranged for a special copy of the Petrarch for that lady, after seeing the manuscript itself.⁵

I am convinced that Aldus never thought at any time that he had tried to reproduce Petrarch's script. His language, to be sure, as quoted by M. Firmin-Didot,⁶ seems plainly to indicate the contrary:

Petrarcha . . . di sua mano così a lasciato alle genti, che doppo lui havevano a venire, in testo diligentissimamente da esso scritto in buona charta, il quale io appo il sopradettovi M. Piero Bembo ho veduto, che altri libri ha di man pure del nostro poeta; e dal quale questa forma a lettera per lettera è levata.

¹ De Nolhac, *Fulvio Orsini*, pp. 283 ff.; A. Pakscher, "Aus einem Katalog des Fulvius Ursinus," in *Zeitsch. f. rom. Philol.*, X (1886), pp. 219 ff.

² For specimens, see *Il Manoscritto Vaticano Latino 3196, autografo di Francesco Petrarca riprodotto in eliotipia a cura della Biblioteca Vaticana*, 1895.

³ See below, pp. 148 ff.

⁴ *Ibid.*, pp. 169 ff.

⁵ Firmin-Didot, *op. cit.*, pp. 370 ff.

⁶ *Ibid.*, p. 163.

Who would blame M. Firmin-Didot for interpreting this passage to mean that Aldus "avait suivi la forme des lettres d'après l'écriture de Pétrarque en imitant les contours de chacune d'elles trait pour trait"?

The trouble is that not enough is quoted here. The words come from Aldus's letter to his readers, "Aldo a gli Lettori" appended to his edition of Petrarch's poetry, "Le Cose Volgari di Messer Petrarca" issued in 1501. In the colophon of the edition,¹ he speaks of the book as "tolto con sommissima diligenza dallo scritto di mano medesima del Poeta, hauuto da M. Piero Bembo." Such language is meant to imply, whatever else it means, that the edition is unusually accurate. Now the letter, "A gli Lettori" is not a part of the original edition. It is not an announcement of its features, including the character of its type. It was appended to the edition after the latter had circulated long enough to be criticized. It is a reply to those critics who had found the edition, despite Aldus's claim, imperfect. He begins:

Io mi credea per certo hauere a bastanza dato fede della correttectione di questo libro, che io ui porgo o lettori; hauendoui una uolta detto, che egli è tolto dallo scritto di mano medesima del Poeta hauuto da M. Piero Bembo.

For all this, the critics had declared, "non essere perciò così compiutamente corretta questa forma che io u'ho data, come si dice." They maintained that he had introduced questionable changes, such *uolgari* for *uulgari*, *canzoni* for *canzone*, *Bauarico* (a somewhat significant change) for *barbarico*. They had also accused him, above all, of omitting some parts of the *Trionfi* and changing the order of others, thus abandoning the pathway of truth:

Ma soprattutto ne triomphi, nequali dicono, che io alcuni capitoli, che si leggono ne gialtri, ho leuati dal mio, et l'ordine mutatone d'alquanti. In tutte le quai cose affermano che io mi sono scostato dal diritto camino del uero.

These critics are insinuating that Aldus has wilfully deserted the archetype. He replies that some of the comments would not have

¹ Impresso in Vinegia nelle case d'Aldo Romano, nel anno. MDI. del mese di Luglio, et tolto con sommissima diligenza dallo scritto di mano medesima del Poeta, hauuto da M. Piero Bembo con la concessione della Illustrissima signoria nostra, che per. x. anni nessuno possa stampare il Petrarca sotto la pene, che in lei si contengono.

been made if the critics had been more familiar with the Tuscan dialect and insists that he is considerably nearer the truth than former editors have been. The point is, once for all, that he is following Petrarch's exact language. If the critics have occasion to find fault, let them cast their aspersions on Petrarch himself:

Tanto solo dico: che se alle uolte cosa, che quiui leggono, nella loro conoscenza non cape; et essi pure ne uogliono riprendere chi che sia, reprendano il Petrarca medesimo, se par loro di ben fare: il quale di sua mano così ha lasciato alle genti, che doppo lui haueuano a uenire, in testo diligentissimamente da esso scritto in buona charta: il quale io appo il sopradettoui M. Piero Bembo ho ueduto, che altri libri ha di man pure del nostro Poeta; et dalquale questa forma a lettera per lettera è leuata in modo, che con pace, di chi mi riprende, in essa non ci ha errori.

This is the entire context and nothing could be plainer. Aldus is speaking of nothing else than the fidelity of his text to the autograph manuscript of Petrarch. This explains why he proceeds to add a list of misprints, "Errori, che stampando si sono fatti." There are twenty-two in all; I should imagine that Aldus discovered them by collating his edition once more with its source. It is, therefore, the phrase "dal quale questa forma a lettera per lettera è leuata," quoted with insufficient context, that has not unnaturally occasioned the general misunderstanding. "Forma" does not refer to the style of lettering. It may mean, like our "form" or "forme," a page of type ready for the press and therefore the size of the page, or "format."¹ Erasmus, as we have seen,² used the Latin *formae* for pages of proof. Aldus is here talking, in a more general sense, of his "impression." "My impression," he declares, "is taken letter by letter — *ad verbum* — from the original." This is precisely the meaning of the word at the beginning of the present address.³ The same idea is conveyed in the language used by Lorenzo of Pavia.⁴ If enough of Aldus is

¹ This is its meaning apparently in the letter of Lorenzo of Pavia. See above p. 93, N. 5 and Firmin-Didot, *op. cit.*, p. 170: "dicti libri cum forma piccola e in letera *canzelarescha* la piu bela vedese mai et è quello che fu ancora inventore de la prima stampa greca, molto mio caro amicho."

² See above, p. 89, N. 4.

³ See above, p. 94: "non essere percio così compiutamente corretta questa *forma*," etc.

⁴ *L'Originale del Canzoniere di Francesco Petrarca. Codice Vaticano Latino 3195, riprodotto in Fototipia a cura della Biblioteca Vaticana (Cod. e Vat. Sel., VI), 1905,*

quoted, he will be found to know what he means and to mean what he says.¹

(c) *Aldus and majuscles*

The above digression will, I hope, be condoned by the reader, for it is not unrelated to my main contention, that Aldus could study forms of script with interest and scientific precision; its importance will also appear in another part of my argument, as we shall see. Just now I mean to say that, after associating with men like Politian and Bembo, Aldus ought to have known what majuscles were even if he did not mention them. But he did mention them, and that, too, in a document which he must have read and revised with some attention, his last will and testament. At the end of the will, after settling the affairs of his estate, and replying to the questions of his notary, he bethinks him of one of the dearest interests of his life, the type that he had invented. This is the final sentence in the will before the signatures of the witnesses:²

Praeterea quia est perficienda quaedam *litera cursiua* quam *cancellariam* appellant, rogo ipsum Andream socerum ut uelit eam literam perfici a Iulio Campagnola ut faciat *maiusculas* quae inter suscribuntur et quae adiunguntur *literis cancellariis*.

p. xxxi: "E se à auto el Petrarcha proprio de man del Petrarcha, conscrito de sua mano, et òlo auto in mane ancora io. Et è de uno padovano che la stima asai, si che l'àno stampato a letera per letera como questo con molta diligencia." See also Firmin-Didot, *op. cit.*, p. 170.

¹ After coming to the opinion above expressed, I was glad to find that it is shared by Mr. Updike. He remarks (*op. cit.*, p. 128): "Tradition says that he intended to imitate the handwriting of Petrarch, — too picturesque a fib to give up, though comparison with Petrarch's handwriting upsets the theory," and adds in a note: "Probably based on a misconception of the phrase, *tolto con sommissima diligenza dallo scritto di mano medesima del Poeta*, occurring in the colophon of the 1501 Aldine edition of Petrarch's *Cose Volgari*. This means merely that the text of the edition has been carefully transcribed from a manuscript in Petrarch's autograph. The statement is twice repeated, in varying phrase but identical meaning, in the address entitled *Aldo a gli Lettori*." I do not think, however, that the false idea would start merely from a misinterpretation of the colophon. It is the phrase in the address divorced from its context that has produced the mischief.

² Firmin-Didot, *op. cit.*, p. 490. A word like *libros* or *capita* seems to have been omitted after *inter*.

The Aldine type, fine as it was, could yet be improved. Aldus requests his father-in-law to see that the improvements are made by an accomplished artist, Giulio Campagnola.¹ He is concerned not only with his *litera cursiua* or *cancellaria*, but with the *majuscles* used for subscriptions; he seems to be using a technical term. The will was written on January 16, 1514 (1515, N. S.), fourteen days before Aldus died. I will not say, lest I be accused of a customary incapacity for moderation, that Aldus murmured "majuscles" with his dying lips, but it is true that it is one of the last words which he dictated to a scribe.

I thus have a feeling, amounting to a conviction, that if a majuscule book like the Morgan Fragment had been placed before Aldus, that scholar was possessed of sufficient palæographical acumen to recognize it as a genuine product of Roman antiquity, written before the later minuscule came into vogue. In other words, to revert to the statement which caused Mr. Merrill to use an exclamation-point, Aldus indeed "might well have known and meant to say that *P* was written in what we should call a majuscule hand and therefore to be dated before, say, the era of Charlemagne." This is, in my opinion, an admirable account of precisely what Aldus would have thought. What "we" call a majuscule hand was called that before us, as I have sufficiently but by no means exhaustively shown, by the humanists of the fifteenth and sixteenth centuries, Aldus included.² That such a

¹ On him, see Firmin-Didot, *op. cit.*, p. 164.

² As we have seen, uncials were not distinguished from capitals in the sixteenth century. The term "uncial" was used, but not in the present sense. Claude Dupuy describes the fragment of Virgil which is now *Vat. Lat.* 3256 as written in capital letters "*quas unciales vocant*" (De Nohac, *Fulvio Orsini*, p. 85). This is the sense of the word as St. Jerome has it (in the preface to his *Commentary on Job*); it seems to mean "inch-high" letters and is the equivalent of Pierius's "*digitales*." The Benedictines Tassin and Toustain, in their *Nouveau Traité de Diplomatique*, 1750-65, were the first to commit the misnomer which prevails to-day; see Steffens, *Lat. Paläog.*², p. v. We need a word to indicate not the size of the letters but their shape. An admirable statement of the difference between the three main kinds of majuscles recognized to-day was made by Leo Allatius in 1642, after he had brought the *Palatinus* (*Vat. Lat.* 1631) of Virgil to the Vatican from Heidelberg (*Cod. e Vat. Sel.*, I, p. 13): *Rotunda autem erat non quae in sphaerae modum obvolveretur a capitali et quadrata diversa sed quae ob artis contemptum et celeriore scriptiōnem, quasi in globulos sed non eos perfectos curvabantur; qualis est hodie in Virgillii codi-*

hand was a sign of a manuscript *venerandae vetustatis* — the phrase is both Aldus's and Politian's — I have also shown. If Aldus assumed too remote an antiquity for his ancient *Parisinus* and "believed uncial to be the mode of writing in Pliny's time," he has, provided that II is part of his book, correctly estimated the general period of its script. He could make an error in assigning a majuscule book to its century and yet be fully aware of the significance of majuscules. One would not say that Pertz,¹ who in 1863 thought the Vatican-Berlin fragments of Virgil a product of the Augustan Age, was incompetent to distinguish the relative dates of majuscule and minuscule. One would not say that a scholar who thought II a Gallic manuscript of the seventh century was thereby incapable of telling whether a book in ancient majuscules was written before Charlemagne or not. The *Romanus* of Virgil has been dated by modern writers all the way from the second century to the thirteenth,² and yet the general sanity and palæographical competence of these gentlemen is probably unimpeachable.

4. Aldus's *Parisinus* clearly an ancient book

Therefore, after a reconsideration of Aldus's words, I regard them as even more significant than they had appeared to me at first. At first, all that I had attempted to show was that his statement was compatible with his actual possession of a veritably ancient manuscript, such as II obviously is. I now think, after weighing the new evidence which I have presented, that Aldus could not very well have spoken as he did unless the *Parisinus* was a truly Roman book. Aldus's language is in the same tone as that which Politian, Pierius and Orsini use when they are discussing manuscripts extant to-day and indubitably of ancient origin. Mr. Merrill states (p. 108) that I imply (quite wrongly) that those who do not think it necessary to consider very seriously the attribution of immense antiquity by Aldus to *P* must be guilty of condemning Aldus as an "arrant knave or fool." I am glad

cibus Vaticanis 3225, 3877 (error for 3867) et alio simili, quem ipse Heidelbergae cum Palatina Bibliotheca in Vaticanam advexi.

¹ Ribbeck, *Prolegomena Critica ad P. Vergili Maronis Opera Maiora*, 1866, p. 227.

² See *Cod. e Vat. Sel.*, II, pp. 1 ff., and Traube, in *Sirena Helbigiana*, p. 307. Even so fine a connoisseur as De Nohac could call the *Romanus* "contemporain de la renaissance carolingienne"; see his "Le Virgile du Vatican et ses Peintures," in *Notices et Extraits des Manuscrits*, XXXV, 2 (1897), p. 4, N. 1.

if my implication over-emphasizes the intensity of Mr. Merrill's animadversions on Aldus, but I submit that my words are not ill-suited to the conditions which prevailed in Aldus's time. If Politian had read Aldus's preface and then discovered that the much-lauded *Parisinus* was what he could have easily recognized as a later minuscule book, his respect for the palæographical competency of Aldus would have been exceedingly slight; the term "fool" would have been strong and Scripturally reprehensible but not inappropriate. If he had then found that Aldus had tried to palm off such a later codex as antique, of the same venerability as the Bembine Terence, the Vatican Virgil and the Florentine Pandects, I can think of language from which a humanist's tongue would not have refrained quite as vigorous as "arrant knave."

For the reasons here presented, I cannot believe that my suggestion of "the precision of the knowledge of the history of palæography possessed by a Renaissance scholar" is "new" to most workers in this field. At any rate, the information is accessible in the well-known and authoritative treatises from which most of my examples are derived.

B. Foundations of the hypothesis that the Morgan Fragment is a part of Parisinus

Aldus's description of his ancient code was my starting-point. I was not "misled," like those persons to whom Mr. Merrill refers,¹ by Aldus's "forensic expressions of praise" into thinking that the unfavorable judgment passed on Aldus by Keil and others was incorrect. It was not alone Aldus's eloquence that impressed me, but its seeming confirmation by the new testimony offered by the Morgan Fragment. For II answers Aldus's account of his *Parisinus* in the following respects.

1. Like *P*, II is undoubtedly a manuscript of venerable antiquity. Dr. Lowe will agree, I am sure, that not as much is known of the history of uncial writing as we should like, but as this is a subject which Dr. Lowe has been investigating for some time, the definite date which he assigns to II is entitled to serious consideration. Mr. Merrill prefers not to sit in judgment on a matter of palæography, but asserts his

¹ *C. P.*, XIV, p. 29.

right, which he certainly has, to criticize a course of reasoning, "even on a palæographical problem" (p. 102). Palæographers will without doubt assent to his exhortations on "the necessity of cultivating a sane agnosticism" (p. 104). But if such agnosticism is so broad as to shelter the possibility that II was written in Gaul in the seventh century, I believe that there is sufficient palæographical material at hand to refute such a surmise.¹ Let us await the verdict of palæographers, including, I hope, some further statement from Dr. Lowe, on the dating "*c. anno 500*" for II and the assumption of Italy as the place of its production. The essential point, once more, is that it was written not later than the sixth century and not very late in that century, that is to say, clearly within the period of antiquity.

2. The second fact on which I built my conclusions is that the Morgan Fragment and the *Parisinus* agree in their external history so far as that may be traced. Aldus's ancient codex came to him from Paris. After he had used it, it disappeared. After what we have seen of the intelligent veneration bestowed on certain ancient books by his contemporaries, I hardly think that it was thrown into the waste-basket, but at any rate it was no more seen. If we were to find it again, Italy would be the most probable place in which to search for it. The Morgan Fragment comes to the United States from Italy, and, as a bit of rare good luck enables us to see, it was once upon a time in France and presumably not far from Paris. For one of its pages (f. 51^r) bears a note in French written in a hand of the fifteenth century with mention of a certain "Jehan de Sauvenières, garde du scel de la provosté de Meaulx, et François Belon, clerc juré de par le Roy."² Moreover,

¹ P. 104: "Fortunately the text-value of the MS. is quite independent of the validity or lack of validity of these conclusions. II might have been written in Gaul, and a century or more later than Mr. Lowe thinks, and still be quite as interesting to one pursuing the study of Pliny's *Letters* rather than of uncial writing." If Mr. Merrill means merely that a fragment written *c. 600* or later would interest students of the text, he expresses an obvious truth but one not particularly helpful in the present connection. Nor would II, if somebody could prove it written in Gaul in the seventh century, fail to be highly interesting, not to say exciting and upsetting, to a student of the history of uncials. And really, on the other hand, a fragment of the seventh century written in Gaul has not *quite* the same text-value as one written in Italy *c. 500*.

² We are indebted to M. Omont for the correct reading of the proper names; see *Bibliothèque de l'École des Chartes*, 1922, p. 474, N. 1.

we perhaps have evidence from a *probatio pennae* on another page (f. 53^r) that the book was in France in Carolingian times; this is not conclusive testimony, but it is worth taking into account. It agrees with a generally admitted feature of the transmission of the ancients' works that our manuscripts of the Latin authors, with very few exceptions, go back no farther than the time of Charlemagne and that the ancient books from which Carolingian copying began were, in the case of most authors, exceedingly few in number.¹

Now it is this scarcity of ancient majuscule books in Carolingian France no less than in the Italy of Aldus's time that lies in the background of some of my reasonings which so distress Mr. Merrill. I am aware that we do not possess a full and accurate list of all the ancient manuscripts extant in France in the days of Charlemagne and his immediate successors. I cannot deny that there *may* have been a dozen ancient copies of Pliny's *Letters* in France, *two* of which were in Paris or its neighborhood in the fifteenth century and thence came down to Italy; but knowing of the journey of Aldus's book and finding that the same journey was taken by the book of which II once was a part, I am still further prone to believe, in the light of the general considerations which I have presented, that the two books were not merely similar but identical.²

3. We turn now to the contents of our fragment. If we found, despite its antiquity and the similarity of its history to that of *P*, that its text differed radically from that of Aldus's edition, we should naturally conclude that the *Parisinus* was still to seek. On the contrary, the text of Aldus and that of II are substantially the same. This fact I showed in detail and need not repeat this demonstration here.³

¹ Traube, *Vorlesungen und Abhandlungen*, II (1911), p. 133. For an admirable survey of the varied accomplishments of Charlemagne, see Dom F. Cabriol and Dom H. Leclercq, *Dictionnaire d'Archéologie Chrétienne et de Liturgie*, III, 1 (1913), pp. 655-825.

² My statement (p. 41) that "like the *Parisinus*, the book to which our fragment belonged had not stayed in Italy" is careless and wrong and is rightly criticized by Mr. Merrill. We surely do not know that *P* was written in Italy. That implication should be deleted from the discussion. The essential point of my argument remains, namely that both II and *P* came down to Italy from somewhere near Paris.

³ One matter that needs clearing up is the curious appearance of *agere* for *facere* (62, 13 = 64, 9). According to Keil, *agere* is the reading of the first edition of 1508.

Mr. Merrill calls particular attention to the brevity of the fragment and states that I try "to make us forget that it is only four Teubner pages in length by talking like a special pleader."¹ I am conscious of no deliberate intention to lead the reader astray. The contents of the fragment are spread before him in our plates and transliterations, and on the very page where Mr. Merrill hears prolonging echoes after the figures which I cite,² I also state that we "are hampered, of course, by the comparatively small amount of matter in II." But we are concerned primarily with the quality rather than with the quantity of the text. I quote the significant readings contained in II; they are amply sufficient, in my opinion, to allow us to assign that text its place with reference to Aldus's edition and, as we shall see, to the classes of the manuscripts. The fact to note at this point in the argument is that Aldus's text is substantially the same as that of II.

C. *Testing the hypothesis*

The above, then, are the facts on which I based my argument. What did I infer from them? That II is certainly a part of *P*? Not at all. My procedure, according to Mr. Merrill (p. 119), is to profess "to be building up a cumulative argument from probabilities, which finally mount up in the total to practical certainty." He then remarks that as none of the items in my argument possess "the actual quality of evidence," the sum of them has the value that one obtains by adding a string of zeros. I have read and pondered Mr. Merrill's indictment of my paper, and am indebted to him for pointing out several errors in details, for which I make acknowledgment at the appropriate places, but he has belittled the evidence presented and not correctly described my method of argument.

What is the evidence thus far? The fact that Aldus's description very probably indicates his use of a really ancient book and the fact that II is such a book do not if combined prove that II was a part of

I called Keil's statement wrong (p. 43), since I found *facere* in the Harvard copy of that edition. But see Mr. Merrill in *C. P.*, XVIII, p. 68, and also my note in the same volume, pp. 348 ff.

¹ P. 106. I admit a careless error in defining the 12 pages of the MS. as 324 lines, when 72 of the 324 are taken up with heading and index to Book 3. Let us say 252 lines, then, and add that for the matter under discussion, the relation of II to *BF*, the index is also of considerable importance.

² *Ibid.*

P, but the information given us by *II* is not totally negligible in our problem; it is not a zero. If we add that *II* like *P* was once near Paris and later came down to Italy, we are considerably strengthening a possibility; we are not adding zero to zero. Finally, when we observe that the text of *II* is substantially that of Aldus, we have still further strengthened the initial surmise.

In such a situation, I arrived at a conclusion which I still think not excessive. My profession is not what Mr. Merrill has attributed to me, but the following (p. 43):

Thus the internal evidence of the text offers no contradiction to what the script and the history of the manuscript have suggested. I can not claim to have established an irrefutable conclusion, but the signs all point in one direction. I see enough evidence to warrant a working hypothesis, which we may use circumspectly as a clue, submit to further tests, and abandon in case these tests yield evidence with which it cannot be reconciled.

Accordingly, I submitted my hypothesis to various tests — all the tests that occurred to me. First, I examined the relation of *II* to the classes of the manuscripts.

1. *II, like P, a member of Class I*

Class I is composed of two chief manuscripts. One is *B*, the *Bellovacensis*, written or at any rate preserved at Beauvais s. IX or IX-X, and now in the Laurentian Library at Florence among the Ashburnham manuscripts (R 98). The other manuscript is *F*, *Florentinus*, also a French book, written probably towards the end of the tenth century; I intimated that both manuscripts might have been written at Fleury. I need not repeat information just here as to the other two classes. It appears that I committed an initial error in supposing that Mr. Merrill intended to designate the *BF* family as Class I. He declares (p. 119):

But I expressly repudiate such summary estimates of the comparative value of MS.-families, and I expressly said that I took them up in the order in which they came to the knowledge of Renaissance scholars. I even refrained from using such designations as Class I, II, III, lest I might be understood.

I am sorry indeed that I misunderstood, especially because it is the duty of a textual critic, as I conceive it, to put a value on readings,

on manuscripts and on classes of manuscripts at every step in his procedure. In fact, though Mr. Merrill objects to "ranking 'Classes' according to some academic marking-system," he can yet declare:¹

Ex tribus ceterum familiis illam quae decem librorum est (this means *BF*) haud dubie ut sincerissimam aestimare oportet, neque autem tam excellens est ut alias aequo animo neglegas.

If there are three classes of manuscripts and one of these seems to an editor the best of the three, what harm is done by calling it Class I is hard to divine.

Aldus's *Parisinus*, by common consent, is of the family of *BF*, that is of Class I, or whatever one may reasonably entitle the best of the classes. At this point I made another statement which I thought innocent but which has likewise attracted Mr. Merrill's fire. "Everybody admits," I said (p. 47), "that the *Parisinus*, as shown by the readings of Aldus, is clearly associated with the manuscripts of Class I. Its contents corroborate the evidence of the title in *B*, which indicates descent from some codex containing ten books." Mr. Merrill, italicizing the words "as shown by the readings of Aldus," emends my "everybody" to "nobody"² and refers the reader to the beginning of his article. The passage meant, I infer, is contained in his second and third paragraphs (p. 98). There we learn that

for nearly four centuries, no one knew anything more about this *P* than what he could get from the title-pages and preface of Aldus and from a few references in the works of Budaeus, or could infer (and that was little enough) from the readings of Aldus in VIII, 8.3-18.11, where (as in X. 1-40) the editor could have had no other source.

After the Medicean codex (*M*), the leading manuscript of what I will venture to call Class II, was discovered, one might infer, Mr. Merrill continues, from the readings in the eighth book — from no other readings, apparently — that *M* and *P* were not of the same family:

Farther than that no one could carry his deductions, though there was some ground for a suspicion that since Aldus so often agreed with *F* in

¹ Teubner edition, p. xx.

² P. 116: "I had not supposed before that anybody, to say nothing of everybody, could build the inference on that foundation. I do not know whom Mr. Rand classes as 'everybody'; I certainly do not think that even through inadvertence I ever made such an 'admission.'"

I-V. 6, and *P* could not have been closely akin to *M* nor (presumably even *a fortiori*) to the eight-book family, it might well have been of the same family as *F*. But this must have been rather guess than inference.

No new light was thrown on the problem until codex *B*, known heretofore only through a few readings noted in the eighteenth-century edition of Korte, was restored to the Laurentian Library and recognized by Havet.¹ Mr. Merrill speaks of the evidence of the title with its mention of ten books and the probable conclusion to be drawn that as the Trajan-correspondence was not preserved, it would seem, in more than one family of manuscripts, *P* and *BF* were of that family. He finally states that this conclusion "was later confirmed by the testimony of the readings from *P* in the Bodleian volume which was the desk-copy of Pliny's *Letters* belonging to Budaeus."

This explanation of why *P* is probably of the family of *BF*, certainly leaves Aldus out in the cold. His readings, according to Mr. Merrill, have nothing to do with the case, except for the *lacunae* in Books VIII and X, and that "is little enough." With Budaeus, we are at last on solid ground.

In this summary of what scholars must have felt about the relation of *P* to *F* and later to *B* before the final light of revelation proceeded from the Bodleian volume, Mr. Merrill fails to mention one discussion of some importance. It is an article by August Otto, not very long but very good, entitled, "*Die Ueberlieferung der Briefe des jüngeren Plinius*"² and written just before Stangl had published many of the readings of the lately recovered *B*.³ Depending merely on the apparatus in Keil's edition, Otto turned Keil's estimate of his classes upside down by proving the superiority of the readings of *F* to those of Keil's leading manuscripts *MV*, those which constituted his *codicum primum genus* (p. vi), a phrase which might be rendered "Class I." Otto pointed out (p. 289) that though *MV* suffered from the intrusion of glosses and deliberate alterations, *F* was essentially free from corruptions of this sort (p. 293). Now in the course of this discussion, Otto makes an elaborate comparison of the readings of *F* and those of the Aldine edition. He remarks (p. 294):

¹ *Revue Critique*, XV (1883), pp. 251 ff.

² *Hermes*, XXI (1886), pp. 287 ff.

³ *Philologus*, XLV (1886), pp. 220 ff., 642 ff.

Den allerge wichtigsten Bundesgenossen hat jedoch der Florentinus in der editio Aldina. Aldus Manutius benützte nämlich zu seiner Ausgabe eine Handschrift, von der er selbst sagt: *est enim volumen ipsum non solum correctissimum, sed etiam ita antiquum, ut putem scriptum Plinii temporibus*. Diese uns leider verlorene Handschrift, aus welcher Aldus den Text an einer Unzahl von Stellen richtig stellte, muss nun ebenfalls der Klasse des Florentinus angehört haben, denn fast an allen zweifelhaften Stellen stimmt die Aldina nicht mit *M*, sondern mit *F* überein; ja die Aehnlichkeit geht soweit, dass auch vielfach die Schreibfehler von *F* in der Aldina wiederkehren.

Here follows a list, declared to be illustrative but not exhaustive, of thirty-five such errors. Otto concludes (p. 295):

Kurz zusammengefasst geht also mein Urtheil über den Stand der Ueberlieferung dahin: wir haben zwei Recensionen der Briefe, die eine vertreten durch *M(V)*, die andere durch die Reste des Riccardianus, *F* und *a*. Keine von beiden ist für die Herstellung des Textes entbehrlich . . . Die in *FRa* gebotene Recension steht . . . dem Original näher als die von *M*.

Verily, this writer seems to cherish more than "a suspicion" that *P* is of the household of *BF*. He apparently does not appreciate that he is dealing in "guesses" rather than "inferences." I had further assumed, since Otto's example in admitting many of the readings of *BFa* has been followed by subsequent editors — Müller, Kukulka and Merrill — that despite whatever distrust of Aldus may have been entertained, the concordance of *BF* and *a* in so many places was one of the signs that *BF* belonged to the same family as *P*. In fact Mr. Merrill himself could once say:¹ "I may remark here that the tradition of *BF*, supported as it so often is by *a*, appears to me generally preferable to that of *MV*." And again:² "Moreover — the first edition issued by Aldus (in 1508) was founded in great measure upon a MS. that contained the ten books, and clearly belonged to the family of *BF*. It is, therefore, worthy of close study, and is here cited as *a*." These two utterances seem to me altogether in the spirit of Otto. I fail to distinguish their meaning from that which I endeavored to express. But I now have a clearer idea of the extent to which Mr. Merrill is at present willing to go in ruling Aldus out of court; we are not to cite the latter's readings as evidence of the relation of *BF* to *P*, and not to suppose that anybody has ever done so.

¹ *Selected Letters of the Younger Pliny*, 1903, p. viii.

² *Ibid.*, p. xlv.

But whatever the evidence — and for Mr. Merrill the chief evidence is furnished by Budaeus — all editors now agree, Mr. Merrill included,¹ that *P* is of the same family as *BF*. Otto, Kukula, and, once upon a time, Mr. Merrill put *a* in the same group, and for aught that I can discover, Mr. Merrill would still put it there. For although *a* can give no information about the readings of *P* (*codex praestantissimus euanuit, neque lectiones eius ex editione Aldina restitui possunt*),² Mr. Merrill cannot deny that Aldus's text is that of the *BF* class. Otto demonstrated that. Now if we consult the Morgan fragment, we find a text undoubtedly of the *BF* sort, as I showed (p. 47). This fact is admitted by Mr. Merrill. "Even a first glance (of any observer who had cultivated antecedent opportunities with even moderate assiduity)" he says (p. 99), "was enough to show that its text was substantially that of *BF*." This is an important admission. Mr. Merrill had convinced himself by a course of reasoning, before he saw the photographs or the collation (p. 100), that "we could know nothing about the character of *P* within the space covered by II, except that its text very probably would have been in close agreement with *F* (and so of course with the later discovered *B*)." At least the tiny fragment can show us what its text is, and we find that it, as would probably be true of *P*, according to Mr. Merrill, can be identified with one of the classes, the best one, of the manuscripts, with which *P* is also connected. This does not settle the case. Mr. Merrill constructs a pseudo-syllogism which he assumes is a summary of my argument and thus has no difficulty in proving it defective.³ But I have constructed no such syllogism. I am engaged in testing a hypothesis. In this first test, the examination of the relation of the text of II to the different classes of the manuscripts, I conclude not only that there is no contradictory testimony but that a new bit of confirmatory evidence has appeared.

There is another point also to observe. Our survey of the manuscripts enables us to say, not only, as before, that the text of *a* is in

¹ Teubner edition, pp. xi ff.

² *Ibid.*, p. xiii.

³ P. 106: "That II is very like *BF* is true; that *P* must have been very like *BF* (as far as they go) is also true; but from these premises no conclusion can be validly drawn that II = *P*, unless a highly improbable circumstance be assumed as an antecedent. But Mr. Rand appears silently to make the presupposition, and hence the argument looks to him like a true and valid syllogism."

substantial agreement with that of Π , but that there is no manuscript in existence to which it stands in a closer relation. Let us add this fact, too, to those which we have collected; its value, in my opinion, is something more substantial than a zero.

2. Π the direct ancestor of BF

Having shown the intimate connection between Π and BF , I next sought to prove that this connection is that of parent and children, or, rather, grandchildren. Here Mr. Merrill endeavors to take the ground from under my feet by declaring (p. 110) that as the evidence for determining that F is independent of B is very minute in amount, though the text to be compared covers one hundred pages, "nothing at all, one way or the other" may be expected "unless by rarest good luck, from four only of these hundred pages, taken consecutively but otherwise at random." But instead of being blocked off by an appeal to pre-ratiocination and the theory of chances, I prefer to examine what these four pages contain.

(a) *Indications in the text*

Certain peculiar errors of BF seemed to me best explained by supposing that these manuscripts descended directly from Π . These cases are presented on pages 48 ff.

(i) *Errors in the text of the letters*

One of my instances seems especially objectionable to Mr. Merrill (p. 111). He takes it "at random as a specimen." He convicts me of making "three errors of observation or of record in a random six-and-one-half lines" and then of constructing an argument "on the two important errors." He adds that he "selected the passage at random after the fashion of the Vergilian (or biblical) *sortes*," and hopes that the fates may have been "tricksy" in guiding him to this particular specimen. It is a superfluous hope, however, since "no elvishness of fates can explain the defectiveness of reasoning" which Mr. Merrill finds "everywhere confronting" him in my paper.

The passage in question occurs on p. 63, l. 23 of Keil's edition (65, 21 Merrill). Here all the other manuscripts have *unice* correctly, while the reading of BF is *uince*. The reading of Π I stated to be

INTICE, "the second hand writing U above I, and a vertical stroke above U." I also stated that in *BF*, "*uince*, the reading of the first hand, is changed by the second to *unice*; this second hand, Professor Merrill informs me, seems to be that of a writer in the same scriptorium as the first." I added that "the error in *BF* might, of course, be due to copying an original in minuscules, but it might also be due to the curious state of affairs in *II*."

I will begin by acknowledging my offences. First, I wrongly referred the passage to Keil 62, 23. Secondly, instead of saying that the second hand added a vertical stroke above the *U*, I should have called this stroke the letter *I*, as Dr. Lowe does; that is obviously what the scribe intended. Thirdly, besides noting that this second hand wrote *U* above the *I*, I should have added that he also deleted the *I*. *Peccavi*.

What remains? According to Mr. Merrill, "out of this (non-existent) mess Mr. Rand tries to squeeze some support for his thesis, by claiming that the reading in *B* and *F* 'might be due to the curious state of affairs in *II*!'" Now what do *B* and *F* have? I have reported them, too; incorrectly, and I acknowledge the error of my ways. They have, wrongly, *uince*, which I stated, incorrectly using the apparatus that Mr. Merrill sent me, was altered by the second hand to the right form *unice*;¹ in reality only *F*, not both manuscripts, show a correction, by a later hand, of the error. Nevertheless, I feel like exclaiming with St. Thomas, "*o felix culpa*!" For the case becomes better for my argument. If the reader will now cast out my errors, start afresh and look at Plate VIII in the Carnegie publication, he will see that the signs of cancellation of the *U* are hard to make out. In his transliteration of this page, Dr. Lowe declares (p. 30) that these signs are "barely visible." He makes this statement, of course, not merely from an examination of the plate — and we regretfully admit that the reproductions are not so good as we had hoped they would be — but from a study of the original manuscript. I can subscribe to his statement with some confidence, for I examined this place and all places showing alteration, with special care. In calling the suprascript *I* "a vertical stroke," what I meant to imply, and what I should have clearly stated, is that the *I* is not of a large and generous character and that it might

¹ Alas! I misinterpreted my own copy of Mr. Merrill's note. The copy was correct, but I misread it.

have been interpreted by a later copyist as a mark of cancellation. This copyist, then, not noting the barely visible cancellation of the initial *I*, could easily get *UINCE* out of what he saw, and that is precisely what *B* and *F* have. The error is an important one for associating these two manuscripts in the same class. Since evidence is at hand, as Mr. Merrill states (p. 100) and as we shall later see, to show that *F* does not descend directly from *B*, it is the common parent of both manuscripts in which the error was committed. Since now we learn from Mr. Merrill that there is no correction in *B* by what might be a contemporary hand, or by any hand, there is less need of considering the supposition that the original of *BF* might have had *unice* in plain minuscules, from which two scribes, *B* and *F*, might possibly have committed the same error independently. If we look back of that parent manuscript to its original for the source of the error, we find in *II* a condition of affairs, whether or not we call it a "mess," which readily accounts for the reading of *BF*.

I believe, therefore, that my serious errors in this matter are two: the first is a wrong reference to Keil, the second is the statement that *B* has been corrected. I might have described the reading of *II* more accurately, but my essential point is not affected by that fact. The castigation of the second of my mistakes does not shake my argument; it strengthens it. The argument, therefore, cannot be said to be "based on errors." I am content with this appeal to the Virgilian (or Biblical) *sortes*.

At the same time, the practice of these *sortes*, as I understand them, consisted in opening at random a whole volume of potentially significant material; the range of choice was wide; the seer was not restricted to three paragraphs on two confronting pages, which is the entire extent of my discussion of the point at issue, so far as the text in the letters is concerned (pp. 48, 49). Had the fates directed Mr. Merrill's attention to the end instead of the beginning of the third of the paragraphs in question (p. 49, l. 23), he would have discovered an instance in which, as I state, the relation of *BF* to *II* seems particularly close. Here *BF* show the curious error, found in no other of the manuscripts reported by Merrill (67, 4) or Keil (65, 7), of *reputare* for *patres conscripti putare*. The error is plainly due to a misinterpretation of the abbreviation P. C. It is not the kind of error that two scribes

would be apt independently to commit, and yet — and this is the point that I took pains to emphasize — it would be an easy error for any scribe to make who copied the very page of II in which the words occur. For this is one of the flesh side pages of our fragment, in which the writing is faded. Let the reader consult the next to the last line of our Plate X, or, if he is lucky enough, the original manuscript; the indistinctness is quite as apparent there.

(ii) *The index to Book III*

The appearance of the index to Book III in II is a highly gratifying surprise. It is one of the features of the fragment that induce me to rate its value high, despite its meagre compass. It adds to the evidence of a very close connection between *B* (*F* lacks the index) and II, for they here agree in most unusual mistakes, which I enumerated on pages 50 ff. In one of them I saw a possible indication that *B* depended directly on II. One of the letters, written to Suetonius, is addressed AD SÜETON TRANQUE, for which *B* has *Adsueton tranqui*. I declared that “the parent of II evidently had TRANQ·, which II falsely enlarges to TRANQUE; this form and not TRANQ· is the basis of *B*’s correction, *tranqui*.” “There is no ‘evidently’ about this,” rejoins Mr. Merrill (p. 113), “unless you have already established the proposition that *B* comes from II.” He fails to get my point. I am speaking in the part of the sentence to which “evidently” refers, solely of the cause of the error in II. I think it “evidently” comes from TRANQ·, first because such an abbreviation would correspond to others in the index — UESTRIC·, SPURINN·, SÜETON — and secondly because, granted this form, it is then natural enough for a thoughtless scribe to write out the usual abbreviation for QUE. The senseless SÜETON· TRANQUE thus resulting would fairly clamor for emendation; therefore *tranqui* is written by *B*. Yet it is a questionable improvement, for it produces an abbreviation the like of which is not found even among the curiosities of the present index. Therefore the reverse process, with TRANQUI· as the original from which TRANQUE was made is most unlikely. Now with thus much of my reasoning Mr. Merrill apparently agrees — that is, he agrees with my “evidently” after all — only that he would derive *B* not from II but from their common source; and further, seemingly to relieve *B* of an indictment for emendation — for he exclaims

at the idea of emendation in *B* (p. 112) — he assumes that some ancestor in copying rustic capitals wrote *I* by a “lucky mistake” for *E*. Whether this explanation is simpler than the one I proposed, I will leave the reader to judge.

(b) *A minuscule copy intervenes between II and BF*

In the text of the letters and that of the index to Book III, the evidence, of which I have here repeated only a part, seemed to favor the hypothesis that *BF* descend directly from II. But though the descent is direct, it is not immediate. A minuscule copy intervenes. That *BF* derive from such a text is partly shown by the instances which I discussed (pp. 49 ff.) and can be proved beyond the shadow of a doubt from the text of these manuscripts outside the portion preserved in II.¹ Again Mr. Merrill does not accurately describe my course of argument. He regards my assumption of this intermediate copy as a desperate expedient for getting out of the difficulties raised by the hypothesis that *BF* descend directly from II.² But this is not at all my procedure. The reader will find, by looking at pages 47 ff., that I attempt to show, in the order that I am repeating here, first that II is a member of the same class as *BF*, secondly that these manuscripts on account of the condition of their text depend directly on II and thirdly, however, that they were copied immediately from an intervening manuscript in minuscules. I am not using this third point to prove the second; on the contrary, I am driven to it from what I know of *BF* quite independently of their connection with II. That is to say, I know from the condition of their text in general that they descend from some minuscule manuscript. If now there is reason to believe that they come directly from II, a minuscule manuscript must have intervened, since II is not in minuscule. I then proceed to test my second point — the hypothesis of the direct descent of *BF* from II — in precisely the way that I am testing throughout my discussion the larger hypothesis that II is part of the *Parisinus*.

¹ I will present the evidence for this statement at a later time.

² P. 112: “Finally, when he is especially hard put to it, he postulates the intervention of a now vanished minuscule between II and *B*.”

(i) *Hypothesis of the intervening copy tested*

To test the new hypothesis, I confronted it with what seemed to me the pertinent facts in the case. We wish to know whether there are any obstacles in the way, any signs that some other hypothesis is more probable. My procedure is thus characterized by Mr. Merrill (pp. 111 ff.):

He has to dispose somehow of a considerable number of disagreements between *Π* and *B*. He accordingly assigns them, now this way, now that, according to the exigencies of his case: some are simple errors due to misreading; some are due to double readings in *Π*; others are emendations readily made by *B* (emendations in *B*!); and finally, when he is especially hard put to it, he postulates the intervention of a now vanished minuscule MS. between *Π* and *B*.

This certainly looks like the performance of a desperate and evasive beggar of the question. But Mr. Merrill has produced this cloud of confusion by throwing together several things which should be kept apart and which I kept apart. Disagreements which consist in errors of *BF* do not in the least disturb the hypothesis that *BF* descend directly from *Π*. Every new stage in the descent of a textual tradition shows new errors. The fact that a scribe makes mistakes in copying manuscript *X* does not prove that he is copying some other book than *X*. These "simple errors due to misreading" are listed on page 52. If Mr. Merrill would assign them to some other cause or show how they injure the supposition that *BF* derive directly from *Π*, I hope he will present his arguments case by case.¹ Again, the instances in which double readings in *Π* are involved I discuss not at this point but where I treat of the close resemblance between *BF* and *Π*. Here is an example (p. 48):

In 61, 14 *BFux* have the erroneous emendation, which Budaëus includes among his variants, of *serua* for *sera*. A glance at *Π* shows its apparent origin. The first hand has *SERA* correctly; the second hand writes *U* above

¹ One error of *B* reported by Mr. Merrill in his apparatus (p. 63) may be stricken from the list. In the title of Book 3, *B* is said to have the curious error EPIS-TV-LARV with the first V corrected to O and a large capital S written above the final V, making EPISTOLARVS. But there is no such S, but merely a small and wavy mark of abbreviation signifying M. See Plate XIII in the Carnegie publication.

the line. If the second hand is solely responsible for the attempt at improvement here, and is not reproducing a variant in the parent manuscript of *II*, then *BF* must descend directly from *II*.

This case is used as possible evidence — my language is duly cautious — to show that *BF* descend directly from *II*. It is not a bit of contrary evidence, a “disagreement” militating against this supposition! If Mr. Merrill will show why *BF*, reading *serua*, might not have come directly from a manuscript which read *SERA* corrected, by an ancient hand, to *SERUA*, I shall be interested in his demonstration. A similar course of reasoning should then be applied to the remaining instances of this kind.

In fine, the cases of disagreement which call for discussion are not those in which *BF* are in error, but those in which they are correct while *II* is wrong. For obviously, if they are right and yet are in direct line of descent from *II*, either these good readings are due to deliberate emendation or are taken from another source. Another hypothesis, which Mr. Merrill favors and to the possibility of which I was not blind, is that *II* and *BF* derive from a common source which had the good readings preserved in *BF* but which were perverted in *II*. Now as I had noted what seemed to me evidence telling enough to incline us to the supposition that *BF* do derive directly from *II*, I am bound either to show that the apparent obstacles are not serious or to abandon the supposition.

How many are these cases? “A considerable number,” one would infer from Mr. Merrill’s statement. He gets his “considerable number,” as we have just seen, by accumulating items which offer no obstacles whatsoever. The readings in point are five in all, two in the index and three in the text; in all of these, *B* and, in the text, *F*, are right and *II* is wrong. I will begin by waiving one of the possible explanations, namely that in the minuscule copy which on other grounds I believed to intervene between *BF* and *II*, certain good readings had been inserted from some other source. Surely, *B* nowhere presents the appearance of a conflated text. I must, then, either reject my hypothesis or explain the correct readings in question as the result of successful emendation. What are the cases?

Two of those in the text, I stated (p. 50), may be readily disposed of: 64, 3 (65, 28) *proferenda*] *conferenda BF CONFERANDA II*; 64, 24

(66, 20) comprobasse] *BF* COMPROUASSE II. These errors I regarded as "simple slips, which a scribe might almost unconsciously correct as he wrote." I still so regard them. The remaining error in the text — 63, 28 (65, 25) si] *BF* SIBI II — I thought and still think, is not difficult to emend if one considers the entire sentence:

Quibus omnibus ita demum similis adolescet, si imbutus honestis artibus fuerit, quas plurimum refert a quo potissimum accipiat.

Now Mr. Merrill exclaims at the idea of attributing emendations to *B* and has justification for his feeling. For the scribe of this manuscript, whose habits Mr. Merrill has studied intimately and repeatedly, is not one of those who tamper with a text. The business of the Mediæval scribe, and in general his practice, is, like that of the printer's compositor to-day, simply to reproduce what is set before him. As I think is true of the first two instances here discussed, he may correct a few obvious details *currente calamo* or slip into "lucky errors," but his purpose is to transcribe. But this is not all that went on in the scriptorium of the ninth century. After the scribe had finished his copying, either he or some special corrector, would, when the work was properly done, compare the copy with the original. Here is the moment when emendation can occur, when the corrector sees, or thinks he sees, that the original itself is faulty. He might also write in, or cause to be written in, variants from other sources of a set of explanatory glosses, some of which later, through nobody's malice prepense, might work their way into the text. There is of course far less deliberate emendation in the manuscripts of the ninth century than in those of the fifteenth, but certainly Mr. Merrill would not deny the possibility of its presence in one of the earlier books.

Now whether or not *BF* descend directly from II, they come immediately from a copy in minuscules; the reader will kindly notice, once more, that I am not making up this information to help me out of a tight place. Moreover, there are signs that the corrector of this manuscript had inserted between the lines either explanatory glosses or deliberate alterations of the text. In 62, 2 (64, 3) imbuare], *B* wrote *imbuaris*, which was changed by *m*¹, a *corrector quidam contemporaneus*, in Mr. Merrill's language.¹ It looks as if in the original of *B*, *imbuaris*,

¹ Teubner edition, pp. vii ff. The correction of *imbuare* may be seen on Plate XIV, col. 1 near the bottom.

or simply the syllable *-is*, had been added above the line either as a gloss or as an emendation, and that *B* had taken it for a correction, his own corrector later reverting to *imbuaire*. If this is not the case, then the scribe of *B* himself has slipped into *imbuaris*, which does not look like a mere accident; if a scribe could make such a change, he surely could emend *conferanda* to *conferenda* and *comprouasse* to *comprobasse*. Such is my regard for the conscientiousness of *B*, a regard shared by Mr. Merrill, that though still believing that scribe capable of making such changes and not particularly reprehensible if he made them, I can more easily suppose that they are the work of the corrector of the original of *B*; this manuscript, even at the risk of naming an infant before its birth, in a fashion disapproved by Mr. Merrill (p. 99), I will call, merely for brevity's sake, Π^1 .

Outside the text comprised in Π , there is a passage most instructive as to the nature of the copy Π^1 , namely

10, 19 (Π , 17) certandum habent] *MVa et m. 1 B* certamen nondum habent *F et m. 2 B*.

Here in Π^1 , the corrector changed *certandum* to *certamen*. I say "changed," because if he had merely written *certamen* above as an explanatory gloss, it is hard to account for the reading of *F* and the second hand of *B*; the latter, called by Mr. Merrill in his Teubner edition (p. viii) an *emendator aetate haud multo posterior* was stated by him in a letter to me to be very probably a writer in the same monastery as the original scribe — in the present case, certainly, he seems to have had Π^1 at his disposal. In that manuscript, the corrector, with *certandum* or more probably *certandū* before him, deliberately emended it by writing \bar{m} above *n* and putting marks of deletion above *ndu(m)*. The change was not made plainly, however, and both *F* and the second hand of *B* thought they saw $\bar{n}d\bar{u}$, that is *non dum*; hence their absurd reading.¹ The corrector of Π^1 , therefore, either has actually tried to emend the text, or, less probably, I think, has inserted an explanatory gloss. In either case, he is following the meaning of the passage with care. Hence I infer that he could have studied the sentence *quibus*

¹ *F* according to Keil has *certam̄ n̄dū*. Mr. Merrill has apparently resolved the compendia. If they are used, it would have been helpful in the present case, though not of course in all cases, to inform the reader of their presence.

omnibus — *accipiat* (63, 28–65, 25) with similar attention and either have deleted *bi* in *sibi* or else written *si* above the line.¹

Of the two cases in the index where a reading of *B* is superior to that of *II*, I have already discussed the address AD SUETON· TRANQ.² Here, if I have analyzed the situation rightly, the correctness of *B* is due to emendation of a sort that presupposes the very error that we find in *II*. This case, therefore, is not contrary evidence against my hypothesis, but an argument for it.

The last case is the correct form *facta* in the title of *Letter XVI*, so given in *B* but erroneously written FATA in *II*. This title with the name of the person to whom the letter is sent is inserted in *II*, so it seemed to me, by the second hand. I was loath to differ from Dr. Lowe on this point, and he may well be right, as the lettering is certainly very similar. Let us call the hand the same as that of the text. It is clear, however, that the insertion was not made at the time the text was written. The pen used is finer, as is appropriate for an inter-linear insertion, and the ink, though lighter in color than the brown used in the other titles, is considerably less faded; it is different ink. The title, *adnotasse videor facta dictaque* is unusually long, as though the scribe, when he came to correct his work, had noticed the lack of title and finding none in the original manuscript had himself supplied the defect, not quite in the style of the other titles. If this is the case — I am not urging this suggestion as proved fact — we have another indication that *B*, which has the same title, derived directly from *II*. The title at any rate is so much longer than the others that *B* has to write the last word on the next line, preceded by a bracket to show that it is included.³

The emendation which I here have to explain is that of FATA, *II*, to *facta*, *B*. Possibly this might have been done by the scribe *currente calamo*. If *B* memorized the whole title, in repeating it to himself he

¹ If *II*¹ had *sibi* represented by the letter *s* with suprascript *i*, it could easily have been read as *si* by *B* and *F*. I am not sure, however, that this compendium for *sibi* occurs in the ninth century. If it does, *II*¹ is probably not of the earlier part of that period. See Lindsay, *Notae Latinae*, 1915, pp. 123 ff.

² See above, pp. 111 ff.

³ Mr. Merrill, Teubner edition, p. ix, omits the bracket and puts *dictaque* two lines below, running it on after the title of Letter 18, *Officium consulatus*. There is no such confusion in *B*; see Plate XIV and for *II*, Plate III.

might by a lucky error write *facta*, on account of the *ct* sound in *dictaque*. Or if the corrector of Π^1 read this title with the care which we saw exhibited before, he could readily have corrected *fata* by turning to the opening of the letter itself.

I find, therefore, that of these five instances of a correct reading in *B* which is not in Π , two may be arguments for and not against my hypothesis, and that in any case, no one of them is weighty enough to over-balance the evidence that *BF* descend directly from Π . I am constantly bearing in mind the well-known scarcity of majuscule manuscripts accessible to copyists in the ninth century. We have such a manuscript in Π and, if my demonstration is correct, do not need to assume the existence of another of its sort.

(c) *External history of Π and BF*

The external history of our manuscripts comports with the hypothesis that *BF* derive directly from Π .¹ *B* was preserved at Beauvais and perhaps was written there. Its script, which seems of the ninth rather than of the tenth century, is not unlike the variety cultivated at Fleury about the middle of that century; but this may have spread to Beauvais a bit later. *F* likewise suggests Fleury, and I am now able to add, fortified by a recent Harvard dissertation, *De Scriptura Floriacensi* by F. M. Carey,² that *F* apparently falls in with an archaistic style of the end of the tenth century. *Bernensis* 136, assigned by Mr. Merrill to the thirteenth century and by Hagen to the end of the eleventh or the beginning of the twelfth, is a direct copy of *F*, as Mr. Merrill states.³ This codex also contains certain orations of Cicero (*Cum Senatui gratias egit . . . Pro Balbo*) edited by Sir William Peterson in the *Scriptorum Classicorum Bibliotheca Oxoniensis*, 1910. This part of the *Bernensis*, dated s. XII–XIII by Peterson, was copied directly from the best of all the manuscripts for these orations, *Parisinus* 7794. This is a book, it seems to me, of about the middle of the ninth century,⁴ in a script which seems not of Tours but of some monastery that had felt the influence of Tours, like Fleury just before the middle of the

¹ See pp. 53 ff., of the Carnegie publication.

² For a summary of Dr. Carey's dissertation, see below, p. 193.

³ Teubner edition, p. xi.

⁴ For a facsimile, see Chatelain, *Pal. des Class. Lat.*, Pl. XXIII.

ninth century. Possibly both *Parisinus* 7794 and the *Bernensis* are products of that monastery. At any rate, the minuscule copy Π^1 , from which *B* and *F* independently derive, was written, it would seem, in some scriptorium of France not too remote from either Fleury or Beauvais.

If Π , therefore, is a part of *P*, and also the direct ancestor of *BF*, it would most probably have been preserved somewhere near the French scriptorium where Π^1 was copied. That Π was near Meaux in the fifteenth century and possibly had been in France from Carolingian times on, we have already seen.¹ The book of which it once was a portion, therefore, would have been accessible, so far as we can see, to the scribe or scribes who copied the original of *BF*. Again let me remark that there were not many books in majuscule in France at that time.

(d) *Text-history of Pliny the elder*

A matter not yet discussed which much needs discussion is the connection between the transmission of the *Letters* of the younger Pliny and that of the *Naturalis Historia* of the elder. The two writers were identified in the earlier Middle Ages and their works sometimes were included in the same volume.² This is true of manuscript *B*, which originally comprised both works. The part containing the *Naturalis Historia* is now in the Laurentian Library in Florence, and, like that containing the *Letters*, has a high place among the manuscripts. The latter portion was sundered from the original volume by the infamous Libri, rejoining its former home but not its former encasement, after a pilgrimage of some years. I have not investigated the matter sufficiently to form an opinion at first hand, but I can at least make one or two suggestions for the future investigator to consider.

There are no less than five fragments in uncial of the *Naturalis Historia* existing to-day. Only one of these contains more than a few leaves, yet all, it would seem, come from different manuscripts which may once have been complete.³ This abundance of ancient copies

¹ See above, p. 100. ² See for instance, Mr. Merrill in *C. P.*, V, pp. 179 ff.

³ For a description of four of them, see Detlefsen, *Rheinisches Museum*, XV (1860), pp. 267 ff., and for recent literature on the subject, Schanz, *Röm. Literaturgesch.*, II, 2, § 493a. The most recently discovered fragment (Autun-Paris) we owe to Chatelain; see *Journal des Savants*, 1900, pp. 44 ff.

might suggest that we should not limit too strictly the number of similar sources for the text of the *Letters*. But in the first place, the *Natural History* was a far more important book for Mediæval needs. It was a treasure-house of information on geography, botany, zoölogy, mineralogy, art and things in general. If it likewise was a treasure-house of misinformation, Mediæval readers were happily unaware of this defect. The marvellous nature of its stories would be no bar to popularity. Who would give up, for instance, the authentic account of the religious propensities of elephants solemnly narrated at the beginning of Book 8? The *Natural History*, like Virgil, was one of the indispensables. We should expect a larger number of ancient manuscripts of such a work to have been preserved than of a collection of letters, whatever their charm.

Moreover, when we examine the extant fragments, we cannot prove from them that a large number of ancient copies actually was available for Carolingian scribes. Before discussing the fragments, I will mention two excerpts from the *Natural History* written in uncials; these copies probably have no bearing on the question but at least should be considered. The first is contained in the *Codex Salmasianus*, Paris *Latinus* 10318, chiefly famous for its text of the *Anthologia Latina*. This book is in uncials, s. VII–VIII, written possibly, but by no means surely, in Spain; it derives from an ancient collection and apparently has no relation to the transmission of the *Natural History* in France. The other excerpt is found in a no less celebrated book, *Codex Lucensis* of the *Liber Pontificalis* and many other things. Immediately before the passage from Pliny is written in the same uncial hand a letter of Alcuin's to Charlemagne, dated between 798 and 803.¹ The script of the book, now uncial and now a rude minuscule,² shows how far Italy was lagging behind France in the development of a book-hand at the turn of the eighth century; work like this had long since been transcended at St. Martin's of Tours. The Pliny from which the excerpt was taken might have been an ancient book, but we know nothing about it. According to Rück,³ the text has a rather close relation to

¹ K. Rück, *Sitzungsberichte* of the Bavarian Academy, 1898, pp. 213 ff.

² Steffens, *Lat. Paläog.*², Plate 41.

³ *Op. cit.*, p. 230. Detlefsen, in a review of Rück (*Berl. Phil. Woch.*, 1898, coll. 1450 ff.) thinks that the latter has put too high a value on the fragment.

codex *D* (*Vaticanus Latinus* 3861). It is not impossible, but it has not been proved, that the original of the *Lucca* excerpt was brought up to France; no trace of that original has been discovered.

If now we turn to the fragments, we note that one of them, *Codex Nonantulus* or *Sessorianus* was found in Italy and, so far as we know, had never been anywhere else. Further, it is palimpsest, as is the longest fragment, *Codex Moneus*, s. V, consisting of 126 leaves, now in the monastery of St. Paul in Carinthia and formerly a book of Reichenau. The character of the script makes this book, in Dr. Lowe's estimation, one of the closest neighbors of II (p. 20). The manuscript is a rare specimen of a *codex ter scriptus*. The text of Pliny, which itself covers a still earlier text,¹ is rewritten with St. Jerome on *Ecclesiastes* in what looks like an early Merovingian hand, earlier, it would seem, than any of the Reichenau hands with which I am familiar.² One might imagine that the Pliny was brought to Reichenau at the time of its foundation in 724 or not long thereafter and there rewritten with St. Jerome. Mone, however, believed that the book, already rewritten, was brought up from Italy by Egino of Verona, who joined the monastery in 796, and Chatelain, apparently accepting this view, regards the overlying script as Italian.³ The codex as rewritten is probably the book mentioned in the Reichenau catalogue of 822.⁴

The Autun-Paris fragment discovered by Chatelain⁵ is likewise palimpsest. Chatelain gives the date of the underlying script as s. IV-V; it belongs, therefore, in the oldest of Dr. Lowe's groups (pp. 17 ff.). It was rewritten with the text of Cassian about 600, and thus would have been in this condition, with the Pliny well concealed, when Carolingian copying started.

The two remaining fragments are *Vindobonensis* CCXXXIII s. V

¹ K. Dziatzko, *Untersuchungen über ausgewählte Kapitel der antiken Buchwesen*, 1900, pp. 103 ff.

² The history of the script of Reichenau in the eighth and ninth centuries presents a fascinating series of problems which richly deserve treatment.

³ *Pal. des Class. Lat.*, Pl. CXXXVI. The hand is not the same as that of II, and the dimensions given by Chatelain do not agree with those of our fragment.

⁴ G. Becker, *Catalogi Bibliothecarum Antiqui*, 1885, p. 6, No. 72.

⁵ See p. 119, N. 3.

and *Parisinus* 9378 s. VI. Both are reproduced by Chatelain.¹ The former seems a bit older and the latter a bit younger than II. The hands are surely different and the dimensions of the Paris fragment do not agree with those of II; the Vienna fragment consists of mere strips. The *Parisinus* was formerly glued to the cover of a manuscript of S. Amand en Puelle, in the diocese of Tournai. There is no indication of the provenience of the *Vindobonensis*.

In fine, of the seven possibilities with which we started, the two last considered may, for aught we know, have been parts of ancient books on which the Carolingian copies depend. There is a slight possibility that a copy might have been taken of the *Moneus* at Reichenau before the codex was rewritten. There is a fainter possibility that the source of the *Lucensis*, whatever that was, became known to Carolingian scribes. The *Sessorianus*, the *Salmasianus* and the Autun-Paris fragment may be definitely ruled out. I would conclude that the number of uncial manuscripts surely available in the Carolingian period for copying a work like the *Natural History* does not encourage us to abandon in the case of a work like the *Letters* the generally admitted assumption that at the beginning of the Middle Ages, no less than in the Renaissance, majuscule manuscripts were exceedingly scarce.

Turning now to the minuscule manuscripts of the *Natural History*, I am impressed with Chatelain's remark that the text of the Autun-Paris fragment is closely related to that of the *Riccardianus*.² The underlying text of a palimpsest cannot of course be the archetype of *R*, but it would seem to be related to that archetype. Chatelain also quotes an observation of Sillig's, whose edition of the *Natural History*,³ whatever its defects, laid the foundations for the stupendous task of sorting and appraising the very numerous manuscripts of a very lengthy work. Sillig cites errors of *R* which indicate descent from some manuscript in uncials.⁴ Some of his illustrations are:

2, 29 ignea vi] igne aut 82 ceu] caeli 173 ut centum] uice-
num 202 ultra] uitro

¹ *Pal. des Class. Lat.*, Pl. CXXXVII. I have photographs of the Paris fragment, kindly furnished me by M. Omont.

Journal des Savants, 1900, p. 48.

³ Hamburg, 1851.

⁴ I, pp. VIII ff.

Now though these errors may be traced to an uncial source, that source might just as well have been rustic capital. In the latter script, *I*, *T*, and *L* could more readily be confused, and the rounded *U* necessary to explain the misreading of *CEU* as *CELI* is found in rustic capital as well as in uncial. Still, to account for the latter mistake, we need an *L* with a rounded base, such as we have in the uncial of Dr. Lowe's later group in which he places II (pp. 19 ff.); this rounded *L* is not characteristic of either the earlier sort of uncials or of rustic capitals. With either script, the errors cited would be possible and with either, something remains to be explained. At any rate, some ancestor of *R*, and not too remote an ancestor, was a majuscule manuscript; if some of its pages were faded, like those in II, mistakes of this kind would be only too liable to occur.

We may now note that these errors have had something of a history. It is possible that a scribe, reading *UITRA* for *ULTRA* should in the act of writing "emend" the erroneous form to *uitro*; but it is more likely that after a slip like this, or like *uicentum* for *UT CENTUM*, had been made, a corrector should introduce what seemed to him improvements, *uitro* and *uicenum*. In other words, we can best explain the text of *R* by assuming a copy between it and its majuscule original. The corrector of that manuscript, like that of II¹, might have made alterations of this sort, *caeli* for *celi* among them, and thus an amount of error would appear in *R* for which the scribes of that copy would not be personally responsible. It is also clear that a second hand in *R* has introduced not only excellent readings from the archetype, but also *foedas interpolationes*.¹ This is curiously like conditions in codex B of the *Letters*, once part of the same volume with *R*. It would be profitable, I believe, to examine palæographically both *R* and *VD*, a closely related manuscript,² to see what they could tell us of the ancestral codices, mediate and immediate, on which they depend. Traube's call for a "paläographisch-kritisch" investigation of the text of the elder Pliny has still its force and still remains un-

¹ Sillig, I, p. IX; Detlefsen, *Rhein. Mus.*, XV, pp. 281 ff.

² Sillig, I, p. IX, and Detlefsen, *op. cit.*, p. 280, show the close relation between *R* and *V* (*Voss. Lat.* F 61), and Detlefsen (p. 273) proves that *V* and *D* (*Vat. Lat.* 3861) were once parts of the same volume. The date assigned to *D* and *V* (11th century) is surely wrong. *V* is reproduced by Chatelain, Pl. CXLI, and is clearly, as he says, s. IX^{ex}.

heeded.¹ Should it prove that the part of *R* which contains the *Natural History* comes immediately from an ancestor in minuscules and directly from one in uncials, we should have the same state of affairs as that which it seems to me probably exists in the case of *B*. Perhaps I had better state that I am not jumping at a conclusion but suggesting a subject for inquiry.

(e) *Theory of the descent of II and BF from a common source*

Mr. Merrill declares that my difficulties would be met by the supposition that *BF* descend not from *II* directly but from a common source. I have tried once more to dispose of these difficulties, and I had tried before to make clear that the difficulties are increased by the hypothesis which Mr. Merrill favors. For (see p. 53) by that hypothesis we must infer that the minuscule original of *BF*, which I will call *X*¹, was derived from a manuscript (= *X*) older than *II*, since the latter descends from that manuscript. If Dr. Lowe is right in calling *II* an Italian book, then so was *X*. Either *X*¹ was copied in Italy or *X* travelled to France precisely as *II* did. Such a theory complicates matters unnecessarily. I maintained, and still maintain, that as we can explain the facts of the tradition by deriving *BF* directly from *II* and regarding *II* as a part of Aldus's *Parisinus*, we may hold to that hypothesis so long as it stands our tests. Mr. Merrill agrees (p. 113) that the points which I allege are all consonant with the theory that *BF* come directly from *II*. He adds that "they do not support it as against the evidently possible alternative that *II* and *B* have simply the marks of a common source." I had called attention (p. 52) to the absence of significant errors in *II* and to their presence in *B*. The definition of "significant" is of course a delicate affair, but I think that few will deny that *reputare* for *P. C. putare* and the other cases that I cite are "significant." They constitute, that is, the kind of error that no two scribes would be apt to commit independently. They are the distinguishing marks of a family or a branch. Any manuscripts which show such errors belong in the same group. We associate *II* with *BF* as members of Class I in virtue of significant common errors which they contain (pp. 47 ff.). Now if *II* had contained errors like in kind to *reputare* for *P. C. putare* and if those errors had been

¹ *Hermes*, XXXIII (1898), p. 351.

avoided by *BF*, it would be hazardous indeed to derive *BF* from *II*. But the errors of *II* avoided by *BF* are not at all of this kind; they are few and simple and such that a subsequent copyist could easily emend. I therefore remarked that we had acquired an impressive sort of negative evidence, and hence should not accept the hypothesis that *II* and *BF* derive independently from a common source, since no significant special errors of *II* could be pointed out. Everything can be explained on the assumption that *BF* come directly from *II*.

Mr. Merrill replies by starting with the theory of the common source as a reasonable explanation of the discrepancies of *II* and *BF* and by refusing to abandon that until I produce some evidence "not that *B* *might* have come from *II*, but that *B* actually *must* have so derived" (p. 113). Now, once more, Mr. Merrill, in a cloud of language which conceals an absence of fact (pp. 111 ff.), implies that the discrepancies are far more serious than they are. Further, though conclusive evidence in a matter of this kind is very difficult to obtain, I have come pretty near it, I think, in pointing out an error of *BF* (*reputare*) for which the faded condition of the page in *II* may well have been responsible. This, with the other indications to which I referred (pp. 48-51), and with the negative evidence just described, warranted me, I believe, in assuming the hypothesis of the direct descent of *BF* from *II* as the most probable hypothesis. I admitted that the amount of material at our disposal is small, but thought, and still think, that my reasoning is correct within the limits that are set for us. A final consideration, and to my mind an important one, is that my hypothesis best fits what we know of the transmission of the ancient authors in the Carolingian age. We should not assume the existence of any more majuscule sources than is necessary. When we examine the nature of Class *II*, as I hope to do in a later article, I think we shall be able to trace it no less than Class *I* to an ancient source. Two majuscule manuscripts must therefore have been accessible in France in the early Middle Ages from which the two classes descend. That ought to be enough.¹ As I have already made plain, I do not here, or

¹ One hypothesis that Mr. Merrill wonders I have not examined is that *P* may have been a descendant of *II*. It would indeed be an interesting hypothesis to examine, if I saw any facts to support it. I started with Aldus's statement, in which my confidence has not diminished but increased, that his *Parisinus* was a truly

anywhere, come to the triumphant conclusion of a syllogism. I am testing an hypothesis to see how long it will run. My words at this point are (p. 54):

This line of argument, which presents not a mathematically absolute demonstration but at least a highly probable concatenation of facts and deductions, warrants the assumption, to be used at any rate as a working hypothesis, that II is a fragment of the lost *Parisinus* which contained all the books of Pliny's *Letters*.

This statement is not heavy with the scent of dogma.

(f) *The hypothesis tested in the text outside of II*

To the argument as thus far presented, I appended a brief treatment of the text without the portion included in II. There is naturally not much evidence to gather here, except what is furnished by such omissions in *BF* as indicate their descent from a manuscript written in lines of the length of those of II (pp. 54 ff.). It is a matter worth examining, another test to apply to our hypothesis, for supposing that we found no such cases as these but on the contrary a dozen or more which clearly showed that *BF* descended from an archetype like, let us say, the palimpsest of Cicero's *Republic*¹ with very short lines, I should have to regard such evidence as weakening the hypothesis. For an archetype would have been indicated that could not be either II itself or the minuscule copy II¹. We should have reached a stage of writing preceding II. It would be possible to surmise that the omissions in question occurred also in the book of which II once formed a part, but there would also be a possibility, and a greater probability than before, of the theory defended by Mr. Merrill that *BF* descend not from II but from a manuscript related to it.

This test revealed five omissions of significance, three of which, I stated, are best explained by supposing that *BF* descend from a manuscript which, like II, had from 24 to 32 letters in a line, while two fit this supposition as well as they do any other. This result did not lead me to a triumphant Q. E. D., but instead I state (p. 57):

ancient codex, and finding in the Morgan fragment a bit of manuscript truly ancient, I am not disposed to regard *P* as a later copy of such a book when the facts, as I see them, can be explained by the supposition that II and *P* are one.

¹ Ehrle and Liebaert, *Spec. Cod. Lat.*, Pl. 4.

Thus the evidence contained in the portion of *BF* outside the text of *II* corroborates our working hypothesis deduced from the fragment itself. We have found nothing yet to overthrow our surmise that a bit of the ancient *Parisinus* is veritably in the city of New York.¹

D. Editorial Methods of Aldus

1. Aldus's basic edition

I next confronted our hypothesis with a new test by considering how Aldus treated his *Parisinus* on the assumption that *II* is a part of that book. The reader will kindly note that the assumption is still an assumption; I am inquiring how it works. I found it natural to suppose that in preparing his edition for the press, Aldus reckoned not only with manuscript sources, *P* above all, but with the editions of his day. Mr. Merrill's surmise that Aldus's pages "would be set up from some one of the recent printed editions, corrected and supplemented into shape,"² seems exceedingly probable. I repeat his statement with no little pleasure, for it is one of the few points on which he agrees with me. In making special mention of the *recent* editions, I infer, I hope correctly, that Mr. Merrill also agrees with me in ruling out two faulty editions that had appeared over thirty years before, the *princeps* (*p*) of 1471 and the Roman edition (*r*) of 1474. It is certainly more probable that Aldus took as his basic edition — I mean by that the edition which he collated with *P* and other sources — one of the later editions, those called *c* by Keil, including that of Pomponius Laetus, 1490, that of Beroaldus, 1498, and that of Catanaeus, 1506. If Aldus had collated a copy of any one of these with *P* and his other sources and turned that over to his printer, the procedure would have been natural enough. I am afraid that we cannot reconstruct Aldus's "copy," at least from the material thus far made accessible. Mr. Hardy thought that he had found it in the famous Bodleian volume consisting of Beroaldus and Avantius, 1502 (Book 10, 41-121), and additions in manuscript of the parts of the *Letters* heretofore lacking.³ This volume, however, after Mr. Merrill's analysis, must be regarded as the property of Guilielmus Budaeus, who had further equipped

¹ On the same page I discuss an orthographical detail which adds to the evidence that a minuscule copy intervened between *II* and *BF*.

² *C. P.*, XVIII, p. 68.

³ *Journal of Philology*, XVII (1888), pp. 95 ff.

the text with a number of marginal and interlinear readings and notes.¹ Nevertheless, though this apparent clue to Aldus's "copy" proved misleading, I think that we should not surrender the search for some edition with which Aldus started and which he modified extensively with the help of *Parisinus*. At least we can say with Keil that Aldus made special use of the 5 editions, and that the first of these builds upon the Roman edition of 1474 as that built upon the *princeps* of 1471.² It is natural that Aldus should maintain the practice of his predecessors in this regard.

2. Aldus's departures from his basic edition

Considering the text of Aldus in the limits of II with relation to the sources of information which we know were accessible to him, I showed that in spite of his relation to the 5 editions, there are 26 readings of theirs which Aldus did not adopt. Let me score one good mark for Aldus now that I may: in all of these cases but six (two of these being titles), his reading agrees with that adopted (naturally from other sources) by Mr. Merrill. I proceeded to ask from what source Aldus drew his corrections of the 5 editions. Some of the 26 readings agree with *p*, and others with *r*, but they are all in Class I (II *BF*) and therefore presumably in *P*. It is natural to suppose that Aldus took them directly from his ancient manuscript by actual collation, instead of picking up some from *p*, some from *r*, and some from other sources. I will return to this point in a moment, and meanwhile invite the reader to examine my detailed account of the 26 passages (pp. 60ff.). Two of the cases (65, 11 and 65, 24) I regarded as crucial because the readings adopted by Aldus are not in any edition, so far as I know, before his time. These, I said, he must have gone to the *Parisinus* to find.

3. On an *F*-codex as a possible source for Aldus

At this point, I have been guilty of a serious act for which Mr. Merrill rebukes me, namely, the failure to mention "the presence among Aldus's forces of some copy of the *F*-text" (p. 109). My only reason for "silently ignoring this matter," and "persistently" ignoring it — the only reason that Mr. Merrill says he can think of — is that "to

¹ *C. P.*, II (1907), pp. 129 ff.

² See the Carnegie publication, p. 58.

concede that Aldus used also a MS. of the F-text would interfere more seriously" with my argument. I can only reply that in this and every part of my work I have sought truth and not subterfuge. It is sometimes possible for a scholar to omit a point not because he is aware that it is dangerous for his theory but because he fails to realize its importance. Mr. Merrill may attribute to me any state of mental incapacity that he pleases but not a deliberate attempt at *suppressio veri*. And it may even be that an airing of the matter which he says I have silently ignored will help rather than hinder my case. Let us see.

As for *F*, I invariably quoted the readings of that manuscript in discussing the twenty-six passages to which I have referred (p. 60). I had also stated the fact (p. 47) that readings of *F* had been utilized in the editions preceding Aldus's, this manuscript and *V* being the only books of considerable antiquity known to the humanists before Aldus's edition appeared. I had likewise noted (p. 46) that Guarino's recension of the Verona manuscript represented a conflation of that text with *F*, and that a text of this sort underlay the *princeps*. I further might have stated that *F* itself continued to be copied, for the first part of the *Letters*, even after Guarino's text acquired a vogue. Therefore my discussion would have been more thorough if I had said that Aldus might have possessed a copy of a straight *F*-text. But in the first place, I really had not understood that we could be so positive on this point. So far as Aldus's statements are concerned, the part of his material that he thinks worth mentioning in his preface comprises (1) the six volumes, some manuscripts, some collated editions, sent down by Fra Giocondo, including his copy of *P*, and (2) *P* itself. That among these sources or others that he used, a straight *F*-codex was surely to be numbered, I had not known. Keil makes no such statement, and the belief in it on Mr. Merrill's part, so far as I can see, has a tendency *crescere eundo*. It appears not at all in his comprehensive article, "The Tradition of Pliny's *Letters*." ¹ In his Teubner edition (p. XIII), speaking of the *alia subsidia* besides *P* at Aldus's disposal, he adds *inter quae fuisse conicimus apographa codicis F. Conicimus* is a safe word; who would cavil at that? At the beginning of his last article, however (p. 100), he declares that "among the several MSS. and collations of Pliny's *Letters* with which Aldus tells us he was

¹ *C. P.*, X (1915), pp. 1 ff.

equipped by Giocondo (and he may have had others also) must *in all probability* have been a copy or collation of *F*, since the *F*-text was widely disseminated in Italy in many copies during the centuries¹ next preceding the time of Aldus, and he could hardly have avoided getting hold of one at least of them, if he had any Plinies at all." Later, in the passage in which he charges me with having suppressed this information, he states (p. 109) that the presence among Aldus's forces of some copy of the *F*-text "is *quite as indubitable* as that Aldus had at command ς or (and) representatives of the Guarinian recension of the Veronese eight-book archetype." Finally, near the end of the article (p. 116), he maintains that "Aldus *indubitably* had an *F*-text at command." The italics in all these excerpts are mine.

If, when I wrote my article, I could have known of this accumulating intensity of conviction on Mr. Merrill's part, I do not believe that I should have silently ignored the possible presence of an *F*-text among Aldus's sources; I should at least have discussed the matter. Yet even now I do not altogether understand why such a manuscript *must* have been in the number. Aldus might have had a whole shelf of manuscripts differing from one another because variously emended but all of the Guarinian recension. Nor does it seem to me that Aldus's possession of an *F*-text is "quite as indubitable" as that of one or more of the ς editions, which were certainly far easier to secure than manuscripts of any description; the chances are as hundreds to tens.

Let us waive these scruples and assume Mr. Merrill's final statement as representing his ultimate belief and the truth. Let us say that Aldus "indubitably" had at command one of the later descendants of *F* when he prepared his edition for the press. What kind of a manuscript was it? Mr. Merrill has said of the later history of *F*:²

Even after Guarino had made known (in 1419) a MS. containing eight books instead of less than five, many justly preferred the old text, so far as it went, to the new; whence it happens that numerous fifteenth-century MSS. of the *Letters* are now extant which faithfully reproduce the text of *F* through V. 6, but from that point on the (always conjecturally emended) eight-book text; in others the eight-book text serves as a basis, but has been more or less freely emended through V. 6 from the *F*-tradition.

¹ See Miss Johnson's lists in *C. P.*, VII (1912), pp. 68 ff. ² *C. P.*, X, pp. 17 ff.

Aldus's *F*-codex, I take it, would at best be a torso or a patch-work affair, copied off in the fifteenth century.¹ Would he have preferred for his chief guide a book of this kind to the *Parisinus*, a *codex venerandae antiquitatis*? For the moment it matters not whether *P* was really ancient or not. Let it be a book in a Gothic hand of the twelfth or thirteenth century. Aldus at least thought it was antique; he could tell at least that it was no fifteenth-century affair; even Mr. Merrill never goes so far — I speak under correction — as to assert that *P* might have been a manuscript of the fifteenth century. Aldus found in his *Parisinus* — and at least part of his statement Mr. Merrill must accept — good readings, improved Greek and important lacunae filled, so that eight books had grown to nine and a tenth had been added to these. Whatever the script of such a book, the value of its contents was self-evident. Would not this, rather than any codex of eight books only, whatever the amount of *F*-text that it contained, have been Aldus's mainstay for his edition?

Let us revert to our 26 readings and state our question in a new form. We are assuming that Aldus had besides *P* a fifteenth-century codex of the *F*-type; I will call it *f*. We also have not forgotten that *P*, by evidence derived in part from Budaeus, is of the same family as *BF*. Seeing now that some of these readings are in *p*, some in *r* and some elsewhere, but all of them in a text like *P* or *f*, did Aldus select them now from this, now from that, now from the other source, or did he take them from a *P-f* text? I should decide for the latter possibility and will cite again, in this modified context, as crucial instances, 65, 11 and 65, 24. These readings Aldus could not have found in any edition; he got them from either *P* or *f*; he is following, in the main, a single source. I will not deny that in cases of difficulty Aldus may have examined many texts, printed and manuscript, to find a satisfactory reading. But when he turns aside from the 5 texts of his day to change the order of two words (61, 12; 63, 9), or three words (63,

¹ I say "in the fifteenth century" rather than in "the centuries next preceding the time of Aldus," since as Miss Johnson's lists show, of the 86 manuscripts that make up the three varieties of *F*-text, there are, excluding the few manuscripts containing excerpts, only two of the thirteenth, two of the fourteenth, and one of the fourteenth-fifteenth century. All the rest are no earlier than the fifteenth century. Mr. Merrill's phrase in his latest article ("widely disseminated in many copies during the centuries next preceding the time of Aldus") might have been more precise.

15), or to leave out an *et* (63, 22), or two *ets* (62, 26), or an *est* (66, 12), or a *iam* (65, 24), or the prænomen *Q.* (66, 11), or to add an *est* (63, 5), or an *in* (63, 23), or an *etiam* (64, 2), or a *quoque* (66, 9),¹ what else is he doing but collating, and minutely collating, a single source which he is determined to follow through thick and thin? And what was this source, a book like *f* or a book like *P*? The answer, to my mind, is plain. The more I ponder this matter, the less dangerous does the assumption of this *f*-codex appear for my theory. I am obliged to Mr. Merrill for calling attention to the point, which deserves discussion but which, I hope he will see, I had not stealthily concealed from misguided motives of prudence.

I think, moreover, that the most potent reason for my neglect was that I had previously studied the relation of Aldus and *F* in the text outside the limits of II, and come to the conclusion that an *F*-manuscript, even of the purest possible species, could not have been his leading authority. I must postpone the full proof of this assertion till my later articles, but to indicate its truth, I will call the reader's attention to a passage already partly discussed.²

10, 19 (11, 17) certandum habent] *MVra et m.*¹ *B* certamen nondum habent *F et m.*² *B* certamen habent *ouxp* 5 certamen nondum (*om.* habent) *m.*² *in ras.* *D* certam dum habeant *m.*

This is Mr. Merrill's apparatus, supplemented from Keil for the editions. Manuscripts *oux* are of the Guarinian recension, while *D* and *m* represent a less emended form of the eight-book text. The accepted reading in Aldus's day — and good enough it would seem — was *certamen habent*. Aldus would hardly follow the authority of *r* for changing to what might well seem a harsher form, *certandum*, but finding this in what he regarded as an ancient and venerable book, he would adopt it as sanctioned by authority. He could not possibly have got it from *f*. The testimony of *m* suggests that the *Veronensis* was correct, but that its reading had become corrupted as in *m*, or, as in *D*, contaminated with that of *F*. It is most improbable, therefore, that Aldus saw *certandum* in a manuscript of the eight-book family or that he would have accepted it if he had so seen it. As it is also highly improbable that he had access to either *M* or *V* and as his

¹ I here have a misprint in citing 63, 9; read *ardentius* for *ardentibus*.

² See above, p. 116.

text in general is not at all of that family, I conclude that he discovered *certandum* by collating his *Parisinus*.

4. *The Parisinus, Aldus's chief source, a better text than BF*

Let this be a crucial instance, then, and let it also show us an important feature of *P*. This manuscript is clearly of the same family as *BF*, but it is a better representative of that family than either of these books. This is obviously true in the case of *F* in the passage just discussed, and though *B* has the right reading in the text, the second corrector, drawing from the archetype, reveals a condition there which prompts to alteration or interpolation, and which elsewhere¹ has led the original scribe of *B* astray.

Now this condition of affairs in *P* is matched precisely by that in *II*. This manuscript, too, is obviously of the *BF* family, but quite as obviously avoids important errors which these two books commit; the reader will find these examined on pages 48 to 52 of my article. This is a new point, and one not unimportant, to add to our evidence that *II* is very probably a part of *P*. It is a new test, which my hypothesis has successfully stood. It is of course only a preliminary trial so far as *P* is concerned. We must wait, to be certain, till Aldus's readings have been observed through the entire text of the *Letters*. The present passage proves, I hope it will be seen, that such an examination is profitable and that despite Mr. Merrill's dictum that the readings of *P* cannot be recovered from Aldus's edition,² it is tolerably certain that they can.

5. *The case of Budaeus*

In treating of Aldus's use of his ancient manuscript, I was naturally involved with the question of Budaeus, especially because, according to Mr. Merrill, Budaeus's "desk-copy" of the *Letters*, the Bodleian volume, contains in its added manuscript portions and in its marginalia our best authority for any part of the lost *Parisinus*.³ For the text comprised in *II*, however, as I showed, Budaeus is most disappointing.⁴ All that he does with the text of Beroaldus, though having *P* itself at his disposal, as Mr. Merrill believes, is to correct two obvious mis-

¹ See above, p. 115.

² Teubner edition, p. xiii.

³ See the references given on pp. 39 ff. of the Carnegie publication.

⁴ *Ibid.*, pp. 59 ff.

prints (*quoque* for *p(er)uoq(ue)* and *Quamuis* for *q Vmuis*), to make an inevitable change in the tense of a verb (*ambulat* for *ambulabat*), and to introduce an unfortunate reading (*serua* for *sera*) which is not in Aldus and is added in Π by the second hand.¹

While Mr. Merrill was settling the case by reasoning of the antecedent probabilities and improbabilities before he saw the photographs of the Morgan Fragment (p. 100), and deciding that "any reading in Aldus within the contents of *F* (I-V, 6) which agreed with *FP* as against other MSS. might just as well have been taken by him from *F* as from *P*," unless one wished to beg the question, he bethought him of what he seems to have judged the final court of appeal. "Of course I turned to my copy of the Budaeus-volume to see whether there were entered on the pertinent pages any readings of *P* that were not also of *F*. There were none." Really, after the list which I have given of Budaeus's corrections, this is not surprising. It is surprising that Mr. Merrill should have regarded such evidence as settling the case. "*Budaeus dixit*" (though not very audibly), "*causa finita est.*" I have just shown, or believe that I have shown, that Aldus and Π can tell us far more about *P* than we here learn from Budaeus. If Π is part of *P*, as according to my hypothesis it is, then we may surely say that Aldus has clung to it tenaciously. His deviations from it are few (p. 42). His text virtually is that of *P*, whereas that of Budaeus is virtually that of Beroaldus.

6. Test furnished by Books 8 and 10

Having found from the tests thus far applied that no fatal obstacle had confronted my hypothesis, I summed up the situation thus (p. 62):

Our examination of the Morgan fragment, therefore, leads to what I deem a highly probable conclusion. We could perhaps hope for absolute proof in a matter of this kind only if another page of the same manuscript should appear, bearing a note in the hand of Aldus Manutius to the effect that he had used the codex for his edition of 1508. Failing that, we can at least point out that all the data accessible comport with the hypothesis

¹ It is in *BF*, and is one of the instances which show the superiority of Π to those manuscripts. Π^1 took the variant instead of the text. Aldus (if Π is part of *P*) took the text and left the variant.

that the Morgan fragment was a part of this very codex. We have set our hypothesis running a lengthy gauntlet of facts, and none has tripped it yet.

Once more, I fail to see that in language like this I have overstepped the bounds of moderation; if this is a syllogism, it is hypothetical and not categorical.

As a sequel to this course of argument, under the heading of "The Latest Criticism of Aldus," I examined a recent article of Mr. Merrill's¹ which seemed to me to present too severe an estimate of Aldus, particularly in view of the general tendency since the publication of Otto's article² to put more faith in the readings of the Aldine edition as true reports of *P*, or at least as desirable features for a text. Mr. Merrill, scrutinizing the parts of the *Letters* included in the manuscript additions to the Bodleian volume, which seem to him taken directly from *P*, found Aldus guilty of deviating from his much-lauded manuscript not infrequently in favor of his own wilful conjectures. If Aldus sins here, Mr. Merrill argues, he cannot be trusted anywhere. My procedure was the reverse of this. Thinking that we have reason for regarding *Π* as a part of *P*, and seeing that if this is so, Aldus has in this section departed very little from his ancient source, I concluded that he followed it closely elsewhere. I admitted that in the parts of the text which had hitherto been unknown in his day, he might have more frequently resorted to conjecture and that in these parts Budaeus and Avantius might be better sources for *P* than Aldus was. I endeavored, on the other hand, to diminish the force of Mr. Merrill's indictment by showing that some of the instances in Book 8 of what he called wilful emendation were nothing more serious than misprints, and that in others, Aldus rather than Budaeus or his scribe has correctly reported *P*. Still, I allowed that even after such discount a significant array of conjectures remains. At this point Mr. Merrill remarks (p. 118):

This, I suppose, must have been an uncomfortable thought, since he drops it at once and flies off on another tack. Nor does he return to it elsewhere. Instead of this, he goes on reiterating his thesis precisely as if this evidence that I presented had nothing to do with the case. I hold it, on the contrary, to be conclusive against his third and final thesis.

¹ *C. P.*, XIV, pp. 29 ff.

² See above, pp. 105 ff.

This third thesis concerns Aldus's use of *P*. A bit later Mr. Merrill adds:

For the establishment of Mr. Rand's thesis the examination of Book X was as necessary as that of Book VIII. After his method it could as successfully have been made. Why did he not attempt it? I am at a loss to decide. It could not have been any bite of inwit.

Verily this is an ignominious and unrepentant flight.

7. *Further testing of the hypothesis*

For all this, I did not intend to let the case go by default. I indicated in my closing paragraph that my argument was not completed, as the testing of Aldus in the entire text of the *Letters* is still to come. I also remarked that even after Mr. Merrill's happy discovery and brilliant analysis of the true nature of the Bodleian volume, the last word on that subject has not yet been said. I had suggested in a footnote (p. 62) that the scribe who wrote the manuscript supplement for Book 8 was copying not the *Parisinus* itself but a copy of it. If this is a fact, it has some significance. It will be evident in Part II of the present paper that I did not design a retreat in the face of overwhelming odds, but merely dramatic suspense.

8. *Results thus far attained*

This may seem an impudent heading now that Mr. Merrill has footed up my arguments and found the sum total *nil*. For all that, there are certain gains for me to count in his own estimate of the case. One is that he now accepts the Morgan fragment as genuine; in our correspondence, even after he had seen the photographs of II, he had expressed considerable doubt on this score, a feeling reflected in his article on Aldus's use of *P*.¹ But no such note of cautious agnosticism is apparent in his present paper. Irreconcilable though our thoughts on other things may be, he now joins Dr. Lowe and me — again I speak under correction — in believing that the Morgan fragment is a genuine bit of ancient uncial.

But this is not all. Mr. Merrill states that there are some parts of my argument which he has read "by no means altogether without sympathy" (p. 105). This sentiment is not obtrusive in his discus-

¹ *C. P.*, XIV, p. 34.

sion of my case, but as he made the statement, I have gone through his paper with hopeful heart and a fine-toothed comb to see what declarations are in my favor. As my gleanings are not abundant, I can only hope that amongst the still unpublished marginalia with which Mr. Merrill has adorned the broad pages of the Carnegie publication, a lonely "*Bene*" now and then appears. If he would assemble these tokens of approbation — it would not take much time — he would encourage me and perhaps help the course of further argument.

But after all, how pleasant was my surprise, like the surprise which readers of Horace's second *Epode* know, when I reached Mr. Merrill's closing words. He states that he is somewhat inclined to the suspicion that II may be a part of *Parisinus*, and that he occasionally practises divination himself. I can ask no more. As I had once hoped that because of my arguments, he might find my thesis plausible, so now I am relieved that despite them he is willing at least to entertain it. Like the drunken man in Chaucer, though slider has been the way and self-hypnotized my condition, I have always aimed for home. Though I have jumped in zig-zag course from conclusion to conclusion and not ensued the straight and narrow path of divination, I have somehow reached the goal. Though I am irreconcilably sundered from my friendly adversary in our fundamental judgments concerning quality of evidence and validity of conclusions, I rejoice if it is the same conclusion at which we have arrived. And if Mr. Merrill will substantiate our common suspicion by arguments better than the unreasonable reasons which I have presented, I will be the first to acclaim him.

II. ALDUS AND BUDAEUS AS SOURCES FOR THE RESTORATION OF THE *PARISINUS*

A. *Aldus's Regard for ancient manuscripts*

Before attempting to account for Aldus's apparently wilful deviations from his authoritative *Parisinus* in editing for the first time the portions of the *Letters* hitherto lost, I will tarry for a few moments, from motives which Mr. Merrill surely approves, with antecedent probabilities. I wish the reader to examine Aldus in his proper setting and to note his aims and habits in general before seeing them exhibited in his manner of editing the *Letters* of Pliny.

1. Aldus's plans for restoring the ancient Classics

It is a mistake to think of Aldus as a modern publisher of the baser sort, interested primarily in the success of a clever business venture. He needed money to achieve his aim; he sought, and in some cases received, the patronage of rich nobles and ecclesiastics, and even appealed to the Emperor Maximilian and Pope Leo X.¹ His father-in-law, Andreas Asulanus, was a financial boon to him, despite the elder gentleman's somewhat frugal disposition. He owned the establishment, to which Aldus contributed nothing but his labor. "*Mensa et tota domus erat Andreae Asulani*," observes Erasmus,² "*Aldus nihil erat nisi opera*." The phrase has a double meaning, for Aldus was industry itself. He hung on his office door a special notice like those which modern men of business sometimes display, only couched in an elegant Latinity, warning bores that they had arrived precisely at Aldus's busiest moment.³ On one occasion, he remarks that he is so engrossed with his tasks, poor man, *ut ne nasum quidem liceat emungere*.⁴ But Aldus's unflagging toil was not devoted to the pursuit of the almighty ducat. His was a programme of high thinking and plain living, the latter part of which was not quite appreciated by Erasmus, who found the Aldine table scanty.⁵ Aldus was not bent on "getting out" an "Everybody's Series" to eclipse numerous competitors already in that field. To see how painfully he lacks the true commercial sense, one has only to compare the advertisement of any wide-awake publisher of our time and place with an Aldine preface. It is a difference of worlds.

Aldus merits a place with the restorers of literature, with the preservers of the Classics, with Charlemagne and Cassiodorus and the scholars of the later Empire who edited the ancients' texts.

¹ A. Firmin-Didot, *Alde Manuce*, 1875, p. 278.

² Leyden edition of the *Opera Erasmi*, IX, p. 1137. This is from a passage another part of which has been quoted above, p. 89, N. 4.

³ *QUISQUIS ES ROGAT TE ALDUS ETIAM ATQUE ETIAM: UT SI QUID EST QUOD A SE VELIS PERPAUCIS AGAS DEINDE ACTUTUM ABEAS: NISI TANQUAM HERCULES DEFESSO ATLANTE VENERIS SUPPOSITURUS HUMEROS. SEMPER ENIM ERIT QUOD ET TU AGAS ET QUOTQUOT HUC ATTULERINT PEDES.* From the preface to *Ciceronis Rhetorica et Libri Oratorii*, 1514, edited by Navagero. See A. A. Renouard, *Annales de l'Imprimerie des Aldes*, 3d ed., 1834, p. 398. The whole preface is most interesting.

⁴ *Ibid.*

⁵ See De Nolhac, *Erasme en Italie*, pp. 31 ff.

Hac Aldus iaceo Romanus mole sepultus
Qua maria Euganeis Adria miscet aquis.
Multa vident nostra librorum millia cura,
Quos tunc dederant tempora longa cibum.

The sentiment expressed in these verses from an epitaph written in honor of Aldus by Camerarius¹ is a commonplace of the times. Perhaps it is forensic laudation. If so, sad to relate, Aldus uses similar terms himself — not that he indulges in self-praise, but that he speaks of his undertaking as that of rescuing the ancients' works from the shades that had enveloped them; this is the ambition which will inspire him to toil on amid hardship and disappointment. Such is his language in the preface to the *Rhetores Graeci*, published in the same year and month as the first edition of Pliny's *Letters*.²

Now Aldus was not the only printer of his day. Doubtless we should be reading the Classics in the twentieth century if he had never lived. But his aims were broader and higher than those of any of his confederates. He was primarily a scholar, the intimate of Politian and Erasmus. His plans were vast. To carry them out he formed his Academy in 1500. It was not merely Venetian or Italian in character but international. With the help of the learned associates united in this common cause, he could state, in the preface to the *Euripides* of 1503, that his press was issuing every month a volume of a thousand copies.³ His staff included experts, none of whom was more noteworthy than Jucundus of Verona, the discoverer of the *Parisinus* of Pliny. Jucundus was a universal genius of the type of Leonardo da Vinci — architect, engineer, mathematician, discoverer of manuscripts, editor of inscriptions and critic of texts, whose multi-fold accomplishments won the plaudits of Politian and Scaliger.⁴ With the help of such confederates, Aldus was indeed putting out a series, the first of its kind in the history of typography,⁵ with precursors, however, as Aldus might not have liked to learn, in the Middle Ages. If we consider the range of authors whom he edited, the volume of Christian poets in 1502 no less than the works of the Pagans, if we remember that he himself composed a Greek and a Latin grammar, on an admirably inductive plan, we are reminded of many a Mediæval collection in-

¹ Firmin-Didot, pp. 421 ff., 430.

² *Ibid.*, p. 314.

³ *Ibid.*, p. 152.

⁴ *Ibid.*, pp. 373 ff.

⁵ Renouard, *op. cit.*, pp. 378 ff.

tended as a "*Vade mecum*" of liberal culture.¹ This aspect of Aldus's undertaking was obvious to Erasmus, who says, in somewhat glowing language:

Intra paucos annos illud futurum polliceor studiosis, ut quicquid est bonorum autorum in quatuor linguis, Latina, Graeca, Hebraica, Chaldaica, tum autem in omni genere disciplinarum, id unius huius opera et plenum habeant et emendatum, nullamque iam literariae suppellectilis partem quisquam desideret.²

This is valuable testimony. Aldus must in all probability have talked over his plans with Erasmus.

But it is the character of the editions which made up Aldus's *bibliotheca* with which our study of the text of Pliny's *Letters* is immediately concerned. Aldus, like most of his contemporaries, was an *ami des arts*, but he was no dilettante. He was interested in the minutiae of script, as we have seen, and of orthography and punctuation.³ He was a careful grammarian and metrist.⁴ If anyone doubts that Aldus set a high value on scholarly detail, let him try to define all the metrical varieties listed for study in the *Grammatica Aldi*, first published in 1501. Aldus was a lover of exactness. We have noted with what pains he saw the *Adagia* of Erasmus through his press, and we have every reason to believe that he exercised a similar care in other cases. In fact this was the general verdict. In a dialogue on *Ragionamento della Stampa* written in 1552,⁵ Francesco Doni, one of the interlocutors, after praising Aldus for his *liberalità* and *grandezza d'animo*, speaks of his *infinita diligentia e patientia in volere egli stesso sempre rivedere e correggere le proprie stampe*. As much is said in a fine verse by Girolamo Bologni, Aldus's contemporary,

*nil nisi correctum formis commendat ahenis.*⁶

But the best testimony of all, apart from the works themselves, is Aldus's own remark in the splendid letter which he sent to Pope Leo X in 1513, invoking his patronage for the Academy.⁷ He speaks of the

¹ See the outline of Miss E. M. Sanford's dissertation below, p. 195.

² *Adagia*, Chil. II, Centur. I, Prov. I (Leyden ed., II, p. 402 E). See above, p. 82; the passage there quoted comes immediately after the one given here.

³ Firmin-Didot, p. 198.

⁴ *Ibid.*, p. 301.

⁵ *Ibid.*, pp. 423 ff.

⁶ *Ibid.*, pp. 427 ff.

⁷ *Ibid.*, pp. 346 ff.

eulogies of his friends, but modestly adds, in the words of Virgil's bashful shepherd, *Sed non ego credulus illis*. He then utters the words that constitute the best tribute to his desire for exact truth: *Nullum enim adhuc dedi librum in quo mihi satisfecerim*.

Aldus's passion for exactness was not applied merely to his proof-sheets. It is the essence of his great plan, which was not only to issue in a convenient and delightful form all the Classics old and new, but to reproduce their exact texts. Moreover — and this is the point of most significance in our present study — the way to be exact is not to exercise one's own ideas of exactness on the ancients. In one of his earliest publications, the *Theocritus* of 1495, Aldus announces what seems like a confession of faith; "*Non enim recipio me emendaturum libros*," he declares. He prefers to print the texts as he finds them and to introduce his own corrections only in the most patent cases. There will be no lack of persons, he wittily observes, who will be ready with their emendations.¹ From this it would appear that Aldus does not mean, in the fashion of the brilliant Poggio and his kind, to venture many alterations of his texts. If we decide, from antecedent probability or the result of investigation, to call him a wilful and violent emender, I cannot escape the conviction, despite Mr. Merrill's remarks on p. 114, that the moral character of Aldus is somehow concerned. Our first desire, naturally, is to recover the readings of *P*. If as a preliminary, we can establish Aldus's reputation as a conservative critic in general, we may be tolerably sure that he did not suddenly change his temperament when he came to edit Pliny. And if our assumption depends in part on Aldus's own statements, we enter the field of ethics when we wonder whether Aldus was inclined to tell the truth.

Still more explicit are the remarks in his edition of the *Cornucopia* of Nicholas Perotti, issued in 1499 and reprinted in 1513. There he declares that it is his purpose to rescue the ancient texts — from what? From the barbarities of Gothic scribes? No, *from the improvements of conjecturing critics*. He is afraid that with the disposition of

¹ Firmin-Didot, pp. 74 ff. M. Firmin-Didot is not blind to this feature of the Aldine texts. "Les éditions d'Alde," he remarks, "ont le grand avantage de nous donner souvent le texte de manuscrits qu'on ne possède plus aujourd'hui; aussi sont-elles toujours consultées des savants éditeurs qui veulent établir un texte d'après les sources originales." Of course, this is an old-fashioned view. Perhaps "*toujours*" is a slip for "*jamais*."

editors to correct, and mutilate, the works that they publish, all the benefits expected from the printing-press will be lost. It had seemed that the mission of the press was not only to make literature widely accessible, but to establish the texts on a firm foundation. What a tragedy if it merely served to perpetuate newly invented error!¹ If the printer was to follow the fashion of the emending scribe, the unemended texts must be preserved before it was too late. Hence the plan of Aldus and his feverish industry in bringing it to pass. His ambition is to save the Classics from the violence of the corrector. Nor is his animosity confined to the mutilators who flourished in his day. He expends his wrath on the ancient sinners who by issuing abbreviated editions of their greater predecessors, contributed to the disappearance of their works. He exclaims:²

Non possum non vehementer irasci audaculis, ac temerariis quibusdam etiam antiquis, qui diminuendis et mutilandis alienis Libris delectati, totam in eo studio curam, operamque posuerunt: id quod, si fecissent, ut sibi inde eligerent quae magis probarent, quaeque facilius memoriae mandare et tenere audacius (*sic* Firmin-Didot — *artius?*) possent, non improbarem eorum consilium; sed ob eam causam id fecisse videntur, ut relictis, ac spretis, tanquam verbosis ac nugacibus propriis Authoribus ipsi laudarentur, ipsi legerentur, oblitteratoque illorum nomine, suum substituerunt. Sic Trogi Pompeii Iustinus; sic T. Liuii Lucius Florus; sic Sexti Pompeii nescio quis

¹ I quote from the edition of 1513: Omne inuentum, quamuis ingeniosum et conducibile adulterari longa die, ac potius malitia hominum, qui se sibi solum natos arbitrari student semper, ex alienis incommodis sua ut comparent commoda, conuerti in malum constat; quemadmodum temporibus nostris accidisse uidemus in miro hoc et quam laboriosis. modo scribendorum librorum. nam quantum quisque commodi ex ea re futurum sperabat, nemo est qui non perspiciat: quantum inde incommodi, quanta bonorum librorum pernicies, quanta ruina et iam sit et futura (nisi Deus prohibeat) uideatur, non queo dicere. Primum enim in quorum artificium manus peruenerint sacra literatura praediti quidam libros omneis enarrare: commentari: corrigere audeant, scimus. Quamobrem periculum non mediocre est, ne beneficium hoc imprimendi libros a Deo immortalis hominibus datum, ipsi cum liceat uel infantissimo cuique pro animi sui libidine temere in quem uult librum grassari in maximum maleficium conuertamus et interitum literarum. Sed de hoc alias, non enim in breui epistola opus esset, si singillatim et cumulate tractare id uelim. Illud nolo silentio praeteriri me, quod in me erit, pedibus manibusque (ut aiunt) facturum, ut laboranti rei literariae consulatur, quemadmodum hoc in libro fecimus. See also the summary given by Firmin-Didot, *op. cit.*, p. 121, which also, apparently, applies to the first edition.

² Firmin-Didot, *op. cit.*, p. 356.

Paulus abbreviator factus est; qui utinam antequam tam iniquum facinus aggrederentur, ipsi uel mutilati, uel discerpti occidissent!

These vigorous words come from one of Aldus's latest works, the edition of Varro, Festus and Nonius Marcellus of 1513. His earlier sentiments had not changed. It would not seem likely that he intended to tamper with texts at any period in his life. His mission was to preserve them in their traditional form.

2. *Codex mihi carior auro*

"Well, my noble Bembo," said Aldus, "the *Petrarch* is out, and here is your special copy, which has been assembled leaf by leaf. The autograph manuscript itself, which you so kindly lent me, I have consigned to Madame Aldus. She finds the old parchment useful for covering her preserve-jars." "My dear Aldus," replied the eminent Venetian with a sign of relief, "Madame is as frugal as she is fair. I am sure that our Paduan friend who owns the manuscript will agree with us that there is no especial reason for preserving the mere copy for your own better printed text, which is a beauty. By the bye, when are you going to get out a Terence? I have an old manuscript of that author on my shelves. Can't you rid me of it? I have ventured the opinion, though some of my friends think me a bit excessive, that it was written in the time of Cicero. It is antiquated, at any rate, or will be when your edition appears."

This is an imaginary scene. It never took place. I have merely tried to dramatize, for vividness, the spirit of several comments by Mr. Merrill on the general disregard of manuscripts on the part of humanistic editors.¹ If I were to describe actualities, it would not be

¹ *C. P.*, II, p. 135: "This wonderful manuscript, like so many others, appears to have vanished from earth. Early editors saw no especial reason for preserving what was to them but copy for their own better printed texts. Possibly some leaves of it may be lying hid in old bindings; possibly they went to cover preserve-jars, or tennis-racquets; possibly into some final dust-heap." *C. P.*, X, p. 20: "But perhaps it is enough to suppose that, as scholars of his (Guarino's) age and a long time thereafter set no particular store upon old MSS. as such, but preferred a supposedly more accurate text, even though it had been secured by modern editing, Guarino cast his old MS. carelessly aside, when once he had in possession improved copies of it." *C. P.*, XVIII, p. 98: "As was the fashion with such things at that period, both this unique MS. (i.e. *P*) and the copy of it passed into the not unfrequented limbo of

discreet to put on paper what Bembo or the gentleman of Padua would really have said. Bembo thought of his Terence as *codex sibi carior auro*. He did not throw it away. It is because he and Orsini, the next possessor, guarded the book so zealously that it is in the Vatican to-day. So is the majuscule Virgil that Bembo possessed. Lorenzo of Pavia states, with a feeling of considerable awe, that he had held the Petrarch in his own hands.¹ The book was not tossed into a dust-heap. Bembo acquired it later and it now is among the treasures of the Vatican. So are numerous other manuscripts once in the library of Bembo. One book for the loan of which Aldus plainly hints was Bembo's codex of the minor poems, or "*Lusus*" of Virgil. At the end of the preface of his *Virgil* of 1514, Aldus says, after much (but not extravagant) praise of Bembo:

Nunc redeo ad Vergilium; cuius lusus quos tu correctissimos habes, tunc edam cum a te accepero; quod erit, ut spero, breui; additis et aliis, quae una cum illis uagari solet, et nostris quibusdam in Vergilium annotationibus.²

This promise of Aldus was fulfilled after his death by his successors in 1534. But they did not cast the *Lusus* carelessly aside, despite its trivial contents. Fulvio Orsini acquired the book, putting it No. 7 in his list, and dating it, too reverently, as a codex "di mille anni."³ One may inspect it to-day in the Vatican Library by asking for *Latinus* 3252. For his *Hesychius*, 1514, Aldus secured the unique manuscript from Giacomo Bardellone. He thanks him for his generosity and states that his honored colleague, Musurus, although in haste, had emended a considerable number of passages, to the great improvement of the text, as anyone could see who compared the edition with the manuscript; the latter had apparently been returned.⁴

'lost' MSS. after Aldus had used them, and later scholars did not bother their heads about their character or fate. Is not a good printed text better far than any one MS.?"

¹ Firmin-Didot, *op. cit.*, p. 171: "Ce manuscrit, moi aussi, je l'ai eu dans mes mains!" See above, p. 95, N. 4.

² I am greatly indebted to Miss Belle Da Costa Greene, Librarian of the Pierpont Morgan Library, for sending to the Harvard College Library for my use the Aldine *Virgils* of 1501, 1514, and 1534.

³ De Nolhac, *Fulvio Orsini*, p. 358.

⁴ Firmin-Didot, *op. cit.*, p. 378. On Musurus's treatment of his manuscript, see below, p. 150.

The great evil in Aldus's day was not the contempt exhibited for manuscripts when once their contents had been appropriated for editions, but the jealous retention of them by selfish owners, who did not wish their prizes to become common property. A few such curmudgeons are left to-day, like the Marquis of Rosanbo, who guards his unique manuscript of Phaedrus, as Professor Postgate wittily says, *Phaedri-anus quidam quasi draco, Phaedro tamquam thesauro incubans*.¹ Erasmus pays his respects to birds of such feather. He felt sure, from what he had seen of Aldus's ancient Pliny, that there must be a number of good manuscripts kept hidden by their owners, whose ambition was to appear the sole possessors of learning.² Aldus, too, in the act of praising Bardellone for his liberality, a trait that Erasmus had appreciated in Aldus himself,³ expresses his opinion of persons of the opposite sort in language almost identical with that of Erasmus.⁴ Moreover, he invents a special title for these stingy owners of splendid books. In a letter sent in 1503 to Cuspinianus (Spiesshammer), the keeper of Emperor Maximilian's library, he exclaims:

Vive autem tu diu uel hoc solo beneficio, Cuspiniane! Vivant et caeteri tales, et rumpantur si qui sunt βιβλιοτάφαι et inuidi!⁵

"Bibliotaphs" — that term might be useful even to-day.

In general, the loss of ancient manuscripts like the *Parisinus* was due, I take it, not to the contempt of printers, the edition once done, but to the cupidity of thieves or miserly possessors. In a footnote (p. 102) quite different, it is pleasant to note, from the spirit of his other utterances on the subject, Mr. Merrill points out that Aldus does not speak as though owning *P* but that more probably "Moceanigo retained his property rights in the remarkable MS., and it was

¹ See the preface of his edition of Phaedrus in *Script. Class. Bibl. Oxon.*, 1919, p. iii.

² See the passage quoted above, p. 82.

³ Venetiam nihil mecum apportabam, praeter confusam et indigestam operis materiam — Aldus nihil habebat in Thesauro suo quod non communicaret. From the passage partly quoted on p. 140, N. 2.

⁴ Firmin-Didot, *op. cit.*, p. 378 translates: "Montrant ainsi par cet acte généreux ton amour pour les lettres et ton désir de leur être utile à présent et à jamais, bien différent en cela de tant d'autres jaloux qui voudraient garder pour eux seuls la science, et se réserver les livres dont il leur plaît de priver les autres."

⁵ *Ibid.*, pp. 220 ff.

given back to him after Aldus had used it." I hope that in the future the footnote will become the text and that such a text as that on page 98 will disappear. Mr. Merrill then asks, pertinently, "Did Mocenigo collect books and MSS., and if so, what became of his treasures? The *Parisinus* of Pliny's *Letters* may have been among them." It is most probable that Mocenigo did collect manuscripts and treasures. His family was one of the most eminent in Venice.¹ Before him, three of his name had held the exalted station of Doge, Thomas in 1413, Petrus in 1474 and Ioannes in 1477. After him another Aloysius was Doge in 1570.² Sansovino, the sixteenth-century Baedeker (and something more) of Venice, in speaking of the private libraries in the city says, "E nobile etiandio per libri Greci et Latini lo studio di Sebastiano Erizo, di Luigi et di Marc' Antonio Mocenighi."³ De Nolhac⁴ gives an amusing account of Orsini's efforts to induce still another Aloysius Mocenigo to part with a beautiful volume of the Provençal poets which was one of Mocenigo's possessions and had formerly belonged to Bembo. Orsini pointed out that though the book was a jewel, its brilliance was unrecognized. It could not compare with the Virgil and the Terence, "li dui in maiuscole, che in tutta Europa non si trovano li (di?) piu antichi, sono poi libri gustevoli da ogni uno." Orsini finally stomached the price and got the book, which after going to the Vatican with the rest of his collection was taken to Paris in the acquisitive reign of Napoleon I.

It is gratifying, therefore, to find Mr. Merrill disposed, after all, to believe that Aldus did not rate his edition so much higher than the *Parisinus* that he allowed the ancient book, in one way or another, to disappear. I can only hope that Mr. Merrill will follow along the clue and find the rest of II — no, no, I mean the closely related *Parisinus* — in Mocenigo's library. Some investigation has been made, I will state, of the antecedents of the Marquis Taccone's collection, without, however, leading to tangible results.

¹ On the Mocenigo family see G. Tassini, *Curiosità Veneziane*, 1887, pp. 470 ff.

² Giovanni Palazzi, *Fasti Ducales*, 1697, pp. 146, 173, 177, 218.

³ Francesco Sansovino, *Venetia Città Nobilissima et Singolare*, 1581, p. 138. Firmin-Didot seems to have no doubt that our Aloisio Mocenigo had a library (*op. cit.*, p. 421).

⁴ *Fulvio Orsini*, pp. 107 ff., 313 ff.

3. Aldus as a collator of texts; the Petrarch of 1501

Let us agree — for I hope that Mr. Merrill will now be concordant on this point — that Aldus would have treated an ancient manuscript with respect and not have destroyed it when the printing was over. But how would he probably — we are still engaged with antecedent probability — how would he probably have ascertained its readings and what use would he have made of them?

Politian, as we have seen, knew how to collate a manuscript with care. He had collated — *contuleram* is his word — a Florence manuscript of Terence with the Bembine and in so doing he declares, had followed his practice of getting its readings exactly, errors and all:

Observauit autem, quod consueui, ut ad unguem exscriberem, etiam quae plane mendosa uidebantur.¹

Aldus had great veneration for Politian. I think they were both of the same school of conservatives. The first of Aldus's editions in his new type, the *Virgil* of 1501, bears on its first page the single word VERGILIUS. The spelling is apparently a tribute to Politian's note in his *Miscellanies*, which Aldus had read, I am more and more inclined to believe, when he edited his works in 1498.² At the end of the *Aeneid* there is a letter addressed by Aldus to the *Studiosi* who read the book. He explains his principle of orthography, showing that certain spellings, like *eis* for *is* in plural cases, are sanctioned by the ancients. He cites Priscian, and however mistaken he is about the details, his intention is to revert to the usage of antiquity. He also explains an elaborate system of accentuation which he has employed, once more on the authority of the ancient grammarians. After the letter, he gives a list of five readings in his edition which he would like to change. The first is:

In primo Aeneidos libro Lauinaq; uenit, scribe Lauinia uenit. Sic enim in antiquissimo codice, Romae in bibliotheca Palatina scriptum est.

¹ See above, p. 86.

² Perhaps, however, Aldus was also aware that the Venice edition of 1472, for which the *Codex Mediceus* had apparently been used, has *Vergilii*. See Sabbadini, *Rheinisches Museum*, LXV (1910), p. 477. The spelling reverts to *Virgilius* in the edition of 1514, of which Navagero was editor.

Sabbadini uses this note to show that Aldus is referring to the *Codex Mediceus* of Virgil, which therefore at this moment of its varied wanderings was in the Vatican.¹ If these readings are typical, Aldus had based his edition on some inferior manuscript, but after somehow finding out about these variants from the *Mediceus*, followed its superior authority. This evidence does not go very far, but it illustrates Aldus's tendency to turn to ancient sources when he can.

But Aldus may be subjected to a thorough testing in the case of one of his editions which professes to be based on a certain manuscript now happily extant. This is the *Cose Volgari* of Petrarch. We have considered this edition in another connection,² and saw that Aldus professed that his text had followed that of its original, the autograph manuscript of Petrarch, *lettera per lettera*. We noted that after his letter to his critics he had appended a list of errors, found, it would seem, by a fresh collation of the manuscript; his critics had charged him with wilful and violent alterations and he referred them to Petrarch's own words.

The history of the manuscript of the *Canzoni*, which was finally acquired by Bembo and passed from him to Orsini and thence to the Vatican, can be traced without difficulty down into the eighteenth century.³ There was no mystery about it; it was catalogued by Rainaldo with his customary care,⁴ and correctly identified by a succession of scholars with *Vaticanus* 3195. The history of the book is not that of its own wanderings. It has always stayed where it was put. The history is that of the wandering fancies of hypercriticism, based on ignorance, as it often is. The tradition that Petrarch's autograph *Canzoniere* was somewhere in the Vatican Library persisted, but the assurance was somehow lost that *Codex* 3195 was the codex in question; the original became confused with the copies. Aldus's reputation for veracity and moderation was naturally at low ebb. He had professed to have followed the author's own copy, which he had seen in Bembo's library, but now it could be scientifically demonstrated that the sup-

¹ *Op. cit.*, p. 478.

² Above, pp. 88 ff.

³ *Cod. e Vat. Sel.*, VI, pp. xix ff.; De Nohac, *Fulvio Orsini*, pp. 279 ff.

⁴ Rainaldo's entry is (*Cod. e Vat. Sel.*, VI, p. xxxv): 3195. Francisci Petrarchae rerum vulgarium opera. . . . Ex Pergameno. c(hartae) s(criptae) n° 72. Antiq. manu propria Auctoris. (*Inventarium MS. lat. Bibl. Vat.*, IV, p. 297.)

posed manuscript was a myth. In 1777, Serassi examined *Codex* 3195 and decided against it on palæographical grounds. In 1824, Blume, in his *Iter Italicum*, declared that the search for the autograph was unprofitable. In 1826, Marsand attempted a philological defence of the ancient tradition, and other scholars followed in his suit, but as late as 1877, Borgognoni proved its impossibility. Carducci the year before, in his *Saggio di un testo e commento nuovo del Petrarca*, breathes no suspicion that the poet's original manuscript might be accessible. A decade later, the Frenchman De Nolhac¹ and the German Pakscher² independently recovered the truth, though De Nolhac was the first to announce it. Controversy did not immediately subside, but the consensus of opinion to-day, as set forth in the elaborate and learned publication of the Vatican authorities, is that beyond question Aldus saw the autograph of the *Canzoni* in the library of Bembo, who had borrowed it at the time and later owned it. De Nolhac declared that Aldus's text is a true reproduction of the autograph manuscript with only insignificant variations.³ Subsequent investigation has shown that the Aldine edition was not printed directly from that codex — that hardly would have been allowed in any case — but its immediate source is a copy of the autograph made by Bembo himself; this is to-day *Vaticanus* 3197. Bembo's manuscript is in part a copy of other sources collated with the autograph, and in part a direct copy of the latter. Some of Bembo's readings are not the *ipsissima uerba* of the poet. Bembo did not always collate exactly and he sometimes teaches Petrarch lessons in spelling and other minor points. But his copy for all that is essentially the poet's own book. The Aldine edition, printed from that copy, is, in the opinion of a most competent scholar, nearer to the genuine text than is any edition that succeeded it down to the close of the nineteenth century.⁴ And whatever Bembo's shortcomings, Aldus performed his part with marked fidelity. His edition fol-

¹ *Op. cit.*, p. 281.

² *Zeitsch. f. rom. Philol.* X (1886), pp. 205 ff.

³ *Le Canzoniere autographe de Petrarque*, 1886, p. 29: Je crois avoir établi l'identité du manuscrit employé par Bembo chez Alde en 1501 et acheté par lui en 1544 avec le *Vat.* 3195. La collation de ce dernier avec l'Aldine corrobore la démonstration: *ils offrent le même texte, avec des différences insignifiantes.*

⁴ G. Mestica, "Il Canzoniere del Petrarca nel codice originale a riscontro col MS. del Bembo e con l'edizione Aldina del 1501," in *Giornale Storico della Letteratura Italiana*, XXI (1893), pp. 333 ff.; *Le Rime di Francesco Petrarca*, ed. 1896, p. xi.

lows with absolute accuracy — *lettra per lettra* — the manuscript which Bembo delivered to him and which was derived immediately from Petrarch's own copy.¹

So then, Aldus's claims for his edition of the *Cose Volgari* of Petrarch, though challenged in his lifetime and later denied by the learned for over a century, now are accepted as in all essentials true. What was once myth and forensic laudation has now become sober history. Possibly there is a fable here for critics of the Aldine Pliny.

4. *Preparation for the Edition of Pliny in 1508*

Our examination of the antecedent probabilities thus far makes it reasonable to believe that if Aldus had possession, or the use, of a genuinely antique copy of Pliny — which he was amply competent to detect as such — he would incline to collate its readings *etiam mendosa* with attention and in general to abide by his ancient authority. I do not mean that he would never have resorted to conjectures, whether made by himself or others. In the edition of Hesychius, 1514, which Musaeus prepared for him, he expressly mentions the latter's emendations and invites the reader to compare them with the imperfections of the codex.² I have allowed conjecture a certain place

¹ Mestica, "Il Canzoniere," p. 329: La relazione è provata ad evidenza dai raffronti che ho fatto . . . tra i due MSS.: basta che nella detta collazione a MS. del Bembo si sostituisca *Edizione aldina* 1501, perchè questa, eccetto i suoi errori materiali segnati per la maggior parte nell'errata-corrige, che Aldo aggiunse in fine del volume, e riproduzione fedelissima di quel MS. A page of *Vaticanus* 3197 is reproduced in *Il Codice Orsini-Da Costa delle Rime e dei Trionfi di F. Petrarca integralmente riprodotto in fotoincisione . . . più tre facsimili dei codici Vaticani*, 3195, 3196, 3197. . . Prefazione di D. Ciampoli, 1904, Plate [III] (before p. 29). The Humanistic cursive script of Bembo would make easy copy for Aldus's printer, but with the kind of corrections which appear on this page, it is something of a triumph to produce an edition of the accuracy which Mestica attributes to the Aldine.

² Hunc autem librum sub tuo nomine in publicum exire uoluimus, Bardellone doctissime, ut et tibi deberent hoc munere studiosi, quem eo gratiorem sibi futurum existimamus, quod eum Musurus compater utriusque nostrum, quantum per occupationes licuit, diligenter recognouit fecitque, licet cursim, *παρὰς ἀπελῶ*. quam plurima enim in eo loca emendata sunt, id quod facile cognoscet qui exemplar ipsum cum nouo hoc conferet (from Aldus's preface). M. Schmidt's edition of Hesychius contains in Vol. V (1868) an excellent account of Musurus by R. Menge. He speaks

in Aldus's edition of Pliny; to what extent his report of *P* is affected thereby we shall examine later. For the moment, I am arguing merely from Aldus's temperament, and his utterances, that he would be apt to collate and accept rather than to improve and invent.

For another reason I think it likely that Aldus would be inclined to adhere to his ancient source, rather than constitute eclectically, after considerable pondering, a text drawn now from various editions of his day, now from a manuscript of the Guarinian recension, now from a fifteenth-century *F*-codex, and sometimes from *Parisinus*. Aldus, as we have seen, was an unusually busy man. His motto for his press, and for his life, was the *Festina lente* of Augustus and Trajan, with its symbol in the anchor and dolphin. Erasmus makes his comment on that *adagium* lead up to the praise of Aldus, the third heir to the symbol and an apt illustration of it.¹ Both parts of the motto apply. In preparing a text, Aldus would seek a secure and ancient foundation, but he would build with all possible speed.

How many years Aldus spent on the text of Pliny I do not know. He must have studied *P* some time earlier than November, 1508, and two years before he could use the codex itself, or at least two years before the edition appeared, the material amassed by Jucundus was in his possession.² This takes us back at the latest to November 1506. Now in the year 1506, Aldus did no printing at all. Venice was in a state

(p. 54) of the very corrupt codex which Musurus used, but warns the reader not to attribute *nimiam intemperantiam atque licentiam* to Musurus. Of the first 2400 glosses, he changed about 550. In over 370 of these his emendations hold to-day; 60 are not accepted; in 4 he spoiled a good reading; in 120, *tales proposuit lectiones quas uiri docti ut non probarunt, ita redarguere non potuerunt*. Menge apologizes for giving statistics, which can not indicate the critical acumen of Musurus; but they at least make clear that the latter *non intemperanter rem egisse*. On this edition see also Firmin-Didot, *op. cit.*, p. 378.

¹ *Opera Erasmi*, Leyden, II (1703), p. 397: Ancora quoniam nauim remoratur et alligat, sistitque, tarditatem indicat. Delphinus, quod hoc nullum aliud animal celerius, aut impetu perniciores, uelocitatem exprimit; quae si scite connectas, efficient huiusmodi sententiam: 'Αεὶ σπεῦδε βραδέως, i.e. Semper festina lente. After speaking of the two Roman Emperors, Erasmus declares: Nunc uero in Aldum Manutium Romanum, ceu tertium haeredem deuenit: Haud equidem sine mente reor, sine numine Diuūm.

² Idque biennio ante quam tu ipsum mihi exemplar publicandum tradidisses (from the preface to Mocenigo).

of war, and he had to close his press and leave the city.¹ A portion of the year was occupied in a search for the *Culex* of Virgil. He encountered great financial loss and could not start printing again till 1507. In that year, only one product of his press is recorded, the translation of two plays of Euripides by Erasmus.² In April, 1508, the second edition of his Latin grammar appeared. From the preface, we see that his thoughts were engaged with Pliny, for he quotes him there twice. But he was also strenuously engaged, as we have seen, with the *Adagia* of Erasmus, which came out in September, and in the same month with the *Pliny*, November, he issued the *Rhetores Graeci* in two volumes. This work was under the special care of Demetrius Doucas,³ but Aldus must have devoted some time to it. And these months were still a time of distraction. At the end of the prefatory letter to Musurus, in the second volume, he quotes Virgil's lines:

Vicinae ruptis inter se legibus urbes
Arma ferunt: saevit toto Mars impius orbe.⁴

Further, the edition of Pliny contained other matter besides the ten books of letters. There was the *Panegyricus*, the *De Viris Illustribus* attributed to Pliny, and the *De Claris Grammaticis et Rhetoribus* of Suetonius. In the last of these texts he had the help of Egnatius, but, he adds, the work had to be done in a hurry — *accurate quidem, sed cursim*, under the sign of the Anchor and Dolphin. To complete the volume, there was the *De Prodigis* of Julius Obsequens, which had come to him from Jucundus.

I mean to imply that while Aldus intended to do a thorough piece of work in the *Pliny*, he could not devote to any one author the years that a modern editor finds essential. Only a part of his plan had been achieved, and great undertakings lay ahead. "*Nam et dedimus multa cunctando et damus assidue.*"⁵ For the *Pliny*, a procedure at once secure and expeditious would be to collate some contemporary edition

¹ Renouard, *Annales de l'Imprimerie des Aldes*, pp. 51, 390; Firmin-Didot, *op. cit.*, p. 283.

² Renouard, *op. cit.*, p. 51; Firmin-Didot, *op. cit.*, p. 293.

³ Firmin-Didot, p. 314.

⁴ *Ibid.*, p. 316. An edition of Aristotle's *Poetics* and one of the *Rhetoric* sometimes ascribed to this year are dubious; see Renouard, *op. cit.*, p. 51.

⁵ Firmin-Didot, *op. cit.*, p. 211.

with the ancient *Parisinus*, generally to accept the reading of the manuscript, and only in cases of doubt to turn to other sources or to emendation. I think we can explain what we find in the edition itself on such a supposition.

But it is time to turn from probabilities to the clear criterion of Aldus which Mr. Merrill has applied in Books 8 and 10, and from which he thought, with some reason, I had hurried away in alarm.

B. *Aldus and the newly discovered portions of Books 8 and 9*

1. *Aldus apparently convicted of conjectural emendation*

The method of convicting Aldus of introducing an excessive amount of conjecture in the hitherto unpublished portions of the *Letters* is at first sight absolutely convincing, especially in Book 8. Here in the Bodleian volume the parts missing in the eight-book family (8, 3 — 20, 1; 9, 16) are supplied by some scribe (*I*) writing under the direction of Budaeus. According to Mr. Merrill, the copy is made straight from *P* itself.¹ Moreover Budaeus has inserted some variants five or six hundred (*i*) from *P*, and a certain number of conjectures from Catanaeus or Aldus or other source. Here, then, are two immediate descendants of *P*, the copy of Aldus (*a*), polished for the press, and Beroaldus's edition revised and supplemented by Budaeus (*Ii*). Which is the more faithful reproduction? A criterion is at hand in the *Mediceus* (*M*), s. IX, the leading manuscript of Class II (the nine-book family), which came to Italy from Korvey not long after Aldus's first edition appeared. No editor had cited its readings, and the *Vaticanus* (*V*), which Pomponius Laetus had used to some extent, did not include the eighth book. There is something dramatic and definitive, therefore, in appealing to the judgment of *M* after *Ii* and *a* have presented their varying testimony. Agreement of *Ii* with *M* would seem to show that their reading is that of *P*. In such cases, as Mr. Merrill puts it, "Aldus abandons indubitably satisfactory readings of his only and much belauded manuscript in favor of conjectures of his own."² This method of catching Aldus in the act of emendation

¹ Teubner edition, pp. xi ff. So *C. P.*, II, p. 156: "For Budaeus doubtless did not depend on Giocondo's copy of the Paris codex, but had the codex itself before him" etc. The italics are mine.

² *C. P.*, XIV, pp. 31 ff.

seems certain and easy of application, or, again to quote Mr. Merrill: "This conclusion does not depend, as that of Keil necessarily did, on any native or acquired acuteness of critical perception. The wayfaring man, though a fool, need not err therein."¹

Let me now sum up the evidence which Mr. Merrill sets in array against Aldus. Part of it is contained in the article in which he announced his discovery of the real nature of the Bodleian volume.² He there gives a list of 47 cases in which Aldus differs from the text, original or added, of the transcript of the missing parts of Books 8 and 10. This list was not composed with the primary purpose of damning Aldus. It was intended to make plain that Aldus did not use the Bodleian volume as copy for his edition, as Hardy had thought. Surely, after Mr. Merrill's exposition, nobody can ever imagine that this was the case. But this list incidentally contains an indictment of Aldus. In his later article,³ Mr. Merrill selected the damaging instances, 25 in number for Book 8, wherein Aldus is proved, by the principle described above, to have preferred his own emendations, and then remarked:

Here is a rather long list of unnecessary, and in some instances particularly flagrant, desertion of *Parisinus* within a comparatively short portion of text. What must have been the case elsewhere? If no further actual evidence were at command, I should hold that this alone was sufficient to prove that Aldus must not be trusted as a loyal, or even a judicious, follower of *Parisinus* in any place where he presents us, as he often enough does, with unsupported readings.

2. *Deductions from Mr. Merrill's list of Aldus's "conjectures"*

Twenty-five transgressions in twelve Teubner pages!⁴ This is a black record — two wilful emendations to the page. I endeavored

¹ Mr. Merrill (p. 119) accuses me here and in another passage of misusing his words. I am sorry if this is so. I can only say that there was no malice in my misinterpretations and that they have not the slightest effect on the main points at issue. As for the present utterance, whether or not it is easy to tell that Aldus is a poor text-critic of Pliny's *Letters*, anybody may be sure, according to Mr. Merrill, that he has wilfully deviated from *P*. Our argument is concerned with nothing else at the present moment.

² *C. P.*, II, pp. 144 ff.

³ *C. P.*, XIV, pp. 31 ff.

⁴ I reckon by the pages in the edition of Kukula, in which the space taken by the apparatus criticus is virtually negligible.

to mitigate Aldus's offence by questioning the relevancy of some of Mr. Merrill's cases. This is the way, according to Mr. Merrill that I did it (p. 118):

But Mr. Rand tries to explain away after his usual fashion as many of the items as possible: "this and that are almost surely misprints; that and the other may be; here the reading of *I* or *i* is an emendation and not the correction of a mistake by restoring the MS. reading (!); this is a misquotation of Aldus; there Merrill fails to remark that Aldus's erroneous reading is supported by *M*." Yet after doing his best he does conclude, "However, even after discounting these and possibly other instances [I shudder to think what he might have done!] a significant array of conjectures remains."

As has happened before,¹ Mr. Merrill has managed to present my items in an amusing and helter-skelter fashion which may readily induce the reader to believe that their sum total has no assignable value. Let us look at them one by one.²

212, 7 (218, 12) fecunditas] *M* foecunditas *Ia*² facunditas *a*
This I called a misprint. The talk is about a pitiful case of abortion. Why Aldus should think the text and the situation might be improved by substituting "eloquence," however intense, for "fecundity," I am at a loss to comprehend. And when I find that in the second edition *facunditas*, which, incidentally, is a very rare form, is changed to *foecunditas*, I conclude that Aldus had not specially selected the former reading in the first edition. I am also gratified to see that this case has been omitted from Mr. Merrill's latest statement of his proof (p. 114). He presents his list again, but 212, 7 is no longer in it. To be sure, he says that he is giving only samples. Perhaps he still regards *facunditas* as one of Aldus's wilful conjectures.

213, 26 (220, 3) pars] *MIa*² par *a*

Par has no sense or construction and is corrected in the second edition. It likewise does not appear in Mr. Merrill's latest list.

These are my two cases of what I consider indubitably misprints. The two very possible cases are these.

¹ See above, pp. 113 ff.

² The page-references given first are to Mr. Merrill's Teubner edition and those in parentheses are to Keil. I will take this occasion to thank Mr. Merrill for calling attention (p. 118) to an unfortunate attempt on my part to correct one of his line-references when he was right all the time.

211, 24 (217, 28) *secedere*] *MI* *sedere aa*²

The context is: *multa me negotia amicorum nec secedere nec studere patiuntur*. *Sedere* I find almost as comic as *facunditas* in the first of these examples, though perhaps the much-employed Aldus might not think the language extravagant. Again I am somewhat encouraged in my suspicion of a misprint on the part of Aldus when I see that Mr. Merrill's printer has played the trick on him, putting *sedere* in his text and ascribing it to *Ia* in the apparatus (p. 114). The latter error is a somewhat curious one to be made by a compositor, but it only shows what curious errors a compositor can make. For all this, since the case heads Mr. Merrill's latest list, perhaps we should infer that what in Mr. Merrill's edition is a typographical slip is in Aldus's a wilful conjecture.

215, 2 (221, 12) *semper fuit*] *MI* *superfuit aa*²

Semper fuit is perfectly plain, *superfuit* a far more difficult construction, to which no consideration tempts. If the former reading came to the compositor in an abbreviated form — *seper fuit*, with a horizontal stroke above *e* — such as Aldus often uses,¹ it would not have been hard for a typographical error to arise.

Perhaps I have somewhat white-washed Aldus by making still more black the inky soul of the printer's devil. But to proceed.

215, 1 (221, 11) *quo*] *M* *quod* (*d del.*) *I* *quod aa*²

Truly *I* agrees with *M* in the right reading here, but only after it has corrected the erroneous reading, likewise that of Aldus, with which it started. Surely it is fair to infer that *a* found *quod* in *P*, which either Budaeus or *I* — it is hard to tell which — emended. Aldus is not the emender here; he is sticking to a possible, though erroneous, reading of his manuscript. He is acting as Catanaeus says he acted. Mr. Merrill puts an exclamation-point after one of my items, which is, as he quotes me, that "the reading of *I* or *i* is an emendation and not the correction of a mistake by restoring the manuscript reading." The present case is an example of such a possibility. Still, though I prefer the explanation which I have given, I hesitate to urge it against an exclamation-point. Let us say that *I*, or *i*, does not here emend but restores the reading of *P* (*quo*). Then *I* and *a* have independently

¹ See the abbreviations used in the two Aldine pages in our Plate XIX, especially *exe(m)pli*, 4 lines from the bottom of the right-hand page.

committed the same scribal error, *quod*. Or shall we say that the *quod* in *I* is a scribal error, as Mr. Merrill evidently believes, whereas the *quod* in *a*, as Mr. Merrill declared in his former article, is one of Aldus's wilful deviations from *P*? I have no means of telling, since this instance does not appear in Mr. Merrill's latest list.

216, 3 (222, 14) periment] perimant *M* perimerent (er *del.*) *I* prement *a*

Whoever did the correcting in *I*, the original reading *perimerent* might well have been in *P*. Aldus, clearly, has emended, but if he found *perimerent*, he did not "desert an undoubtedly satisfactory reading" of his ancient manuscript. He emended in one way, and Budaeus, or *I*, in another. Of course it may be that *P* was correct and that a scribal error by *I* was corrected from *P* by Budaeus. But Mr. Merrill cannot say that it was "indubitably" so. It would almost seem as if he saw that the evidence of this case is not very strong; at any rate, it is not in his latest list.

213, 9 (219, 17) solo] *I* Cat.² soleo *Maa*²

Here *I* backs up a good conjecture by Catanaeus. *Soleo* does not look like a deliberate emendation; it yields some sense but not much. We observe that *M* has it too. Now that manuscript is not marred by wilful emendations; it shows incorporated glosses, but that is the result of a slow and unpremeditated growth. Finding *soleo* in *M*, we should call it a scribal error. Must it be a conjecture in *a*? I had briefly referred to this case (p. 64) as one in which Mr. Merrill "fails to remark that Aldus's erroneous reading is supported by *M*." To this Mr. Merrill replies in a footnote (p. 118): "Of course I did not mention it, for it had nothing to do with the case." I think it has some pertinence; the appearance of the identical error in a manuscript like *M* prevents us from saying that Aldus's reading is surely a conjecture. But I am glad to find that the instance as a whole has not so much to do with the case as Mr. Merrill first thought, for he fails to include it in his latest list.

Thus far I have succeeded in not-crossing seven of the twenty-five charges against Aldus. That is all that is incumbent on me just here. Mr. Merrill must make good his proof in every instance; but in these seven instances there are grave doubts confronting his simple rule that a deviation in *a* from the united testimony of *I* and *M* convicts Aldus

of a wilful and flagrant desertion of his ancient codex. Curiously, five of the seven have no place in Mr. Merrill's latest list; one of the two would also have been omitted, if Mr. Merrill had not still preferred to ascribe a wilful conjecture to Aldus for a reading that in his own edition he declares a misprint. I can hardly think that in excluding so many of the readings which I had called in question Mr. Merrill has listened to my unreasonable reasonings on the point.

But I will cease, for the moment, to defend Aldus, and admit that of the cases that remain, there are at least ten sure instances of what according to Mr. Merrill's principle must be attempts at emendation. Granting his premises, Mr. Merrill's conclusion is sound. If Aldus deliberately abandons his ancient witness as often as this, one must be chary of accepting his unsupported readings elsewhere as those of *P*.

3. *The script and text of I*

I must ask the reader not to be put out of humor, if I now shift the point of attack. For, as I intimated, I have new data to present, derived from an inspection of rotographs of *I*. For these I am indebted to the courtesy of Mr. A. Cowley, Bodley's Librarian, and to the kindly offices of Dr. Lowe. Accurate information had not been accessible to me until the rotographs came, Mr. Merrill's edition not having appeared. In revising my argument, for the present paper, I have of course consulted and utilized Mr. Merrill's edition at every point.

The scribe of *I* writes a fair, clear Humanistic book-hand, in which he tries to approach as nearly as possible to the type of Beroaldus's edition. (See our Plate XVII.) He himself makes some corrections, and others are made by Budaeus. An interesting feature of these latter corrections to which Mr. Merrill has called attention¹ is that they are written in two different hands, or rather, according to Mr. Merrill, by Budaeus at different times. With only the rotographs at my disposal, it were unprofitable to examine this matter further or to try to distinguish the different varieties of *i*.

I, like most scribes, is not impeccable. In all, he has committed some 27 errors, most of which have been corrected by *i*. I am not reckoning the mistakes which the scribe himself makes good; in some

¹ Teubner edition, pp. xiff.

cases it is hard to tell who has done the correcting. One of the most important errors is:

214, 14 (220, 22) quod censentibus ius quae uis magistratibus] *Ma om. I add. marg. i.*

A few uncorrected errors remain in *I*.

214, 21 (220, 29) nulla, nulla] *Ma nulla I.*

214, 31 (221, 10) simus] *Ma scimus I*

Here *i* does not correct an obvious mistake.

215, 7 (221, 18) si] *Ma om. I*

215, 16 (221, 27) absoluit] *M Merrill absoluerit a soluit I Bud.*

Here the error of *I* consists in the omission of *ab*. It is in itself a trivial mistake, but as it appears in Budaeus too, it becomes a not unimportant indication that he drew his text from this very volume. Aldus's *absoluerit* seems to me a compositor's error, since the following *occidit*, which is in the same construction, remains unchanged and since *absoluere* in the preceding line would readily give rise to the error.

218, 28 (225, 5) operit] *Ma aperit I*

A pardonable error is committed by *I* in omitting *que* in 220, 27 (227, 4) lauandos fricandosque] *Ma lauandos (lauando i) fricandos I*

Here is a case, for once, where Mr. Merrill (as I interpret him) would admit that the reading of *P* (a humble one) may be recovered from Aldus, since the latter is confirmed by *M*. But, of course, this may be merely one of the wilful conjectures, though successful. At all events, whatever the reading of *P*, Budaeus has been caught beyond peradventure in an attempt (not a happy one) to emend. Another case just as patent is 217, 7 (223, 18) an iri] *Mia et iri i.*

There is no apparent effort at emendation on the part of *I*. One or two changes might look like emendations, but are due rather to the proximity of grammatically similar forms or to the suggestion of grammatical possibilities. In such a case, the scribe, after memorizing a few words, is led, for the above reasons, to confuse forms or constructions. It is not an error of sight or of hearing — he may have seen and then in repeating to himself heard correctly — but rather of memory and mental confusion. He has dictated to himself twice, once correctly, when he read the words, once incorrectly, when he repeats them to himself as he puts them on his page.

In this category, I should say, belongs

213, 6 (219, 13) causa referendae] *M* cause (*corr. i*) referend (a)e
I caussa referenda *a*

Aldus's reading spoils good sense, but unlike that of *i* proceeds, *I* suppose, from his determination flagrantly to emend.

217, 8 (223, 18) oportuerit] potuerit (*corr. i*) *I*

I, at any rate, does not intend to improve the text that he is copying. He reproduces it as carefully as he can, rightly paying little attention to the sense. But he falls into some curious mistakes:

214, 29 (221, 8) tulimusque] tulistulimusq(ue) (tulis *expunct.*) *I*

218, 19 (224, 28) amici sinu] amicissii sinu (ssii *expunct.*) *I*¹

Then there are several repetitions, as

216, 21 (222, 23 a duobus quae non uideretur una] a duobus quae non uideretur a duobus una (*posterius a duobus expunct.*) *I*

217, 1 (223, 13 potest] potest potest (*posterius potest expunct.*) *I*

Such slips indicate a writer who is copying his text fragment by fragment without considering the relation of the words to one another. At times he appears to have memorized too much, and thus to have been involved in the kind of confusion illustrated above and in the following passage.

214, 31 (221, 10) felicius] facilius Foelicius (facilius *expunct.*, o *del.*) *I*

It should be added that while *I* is not a professional emender, he does, like Aldus, adopt various of the spellings of his day — *tanquam*, *utrunque*, *circumscripito*, *nunciantur*, *hospicium*, *laetitia*, *preciosa*, *solatia*, *charissimorum*, *prophani*, *foecunditas*, *foelicius*, *foeminas*. Few if any of these forms would have been found in an ancient book like *P*. The hand *i* has *absolventeis*, a spelling which, as we saw, Aldus introduced in his *Virgil* of 1501 after his study of Priscian.

4. Correct readings of Aldus supported by *I*

So much for the habits of the scribe *I*. We will examine later the character of his text, and return now to Aldus. As Mr. Merrill has revealed the iniquities of our editor, it is only fair to point out that in a number of crucial cases, Aldus will be found in agreement with *I*, that is, according to Mr. Merrill, with *Parisinus*. I begin with certain

¹ Mr. Merrill reads *amicissu*, not *amicissii*. The dots over *ii* are not visible in the rotograph, but the two strokes are not connected at the bottom as they would be in a *u*.

readings in which Keil, daringly from his point of view, accepted against *M* the reading of Aldus, which is now confirmed by *I* as that of *P*. Mr. Merrill's text agrees with *Ia* Keil unless otherwise stated. I do not include certain trifling errors of *M*, such as misspellings, of which there are some 20 cases.

211, 12 (217, 14) tantum infra etiam] *Ia om. M*

211, 17 (217, 16) et] *Ia om. M* 211, 25 (217, 29 sunt] *Ia om. M*

212, 10 (218, 15) nomina] *Ia omnia M*

212, 20¹ (218, 25) iam hilaris] *Ia hilaris M*

212, 24 (218, 30) aut nepte] *Ia. om M*

213, 1 (219, 8) multorum] *Ia multorumque M*

213, 9 (219, 17) in quo] *Ia in qua M*

213, 13 (219, 22) ergo] *Ia ego M*

214, 7 (220, 13) autem] *Ia ante M*

214, 20 (220, 28) cum suspecta] *Ia cum inspecta (cum in ras., ut uidetur, m¹) M*

216, 14-15 (222, 26-27) in hanc partem, qui alia omnia, in illam partem ite, qua sentitis] *Ia om. M*

216, 20 (222, 32) dubium] *Ia durum M*

216, 24 (223, 3) tantum sollemnibus] *Ia tantum (corr. ex tantum lemibus M > 216, 29 (223, 8) absolventibus] Ia ab / absolventibus M*

216, 33 (223, 11) quae] *Ia qui M*

217, 1 (223, 12) comprobata] *Ia conparata M*

217, 1 (223, 13) qua ergo ratione] *Ia quae ratione M*

217, 13 (223, 23) ne si] *Ia nisi M*

217, 14 (223, 24) absolvendos] *Ia solvendos M*

217, 19 (223, 30) altera tertia] *Ia alteram tertiam M*

218, 15 (224, 24) sapientes uideri qui an magni] *Ia om. M*

218, 16 (224, 25) dolore] *Ia dolere M*

218, 29 (225, 5) Anio] *ai Auio I ante M*

218, 30 (225, 6) uillis] *Ia illis M*

218, 33 (225, 10) euexit] *a eiecit M, Merrill*. On the reading of *I*, see below, p. 173.

219, 3 (225, 14) atque culmina] *a om. M, Merrill*. On the reading of *I*, see below, p. 174.

¹ Mr. Merrill, in his apparatus, has 19, wrongly, for 20.

- 219, 4 (225, 15) ad] *a marg. I om. M*
 219, 11 (225, 22) paruulum] *Ia paruum M*
 219, 22 (225, 33) inexpectata] *Ia exspectata M*
 219, 26 (226, 5) quos] *Ia quod M*. See on the whole passage
 p. 176 below. 219, 28 (226, 7) filiae] *Ia ille M*
 220, 6 (226, 13) et quidem cum opibus amplissimis] *Ia om. M*
 220, 10-11 (226, 18) ut patris . . . curauerit] *Ia ut patres . . . cura-*
uerint M 220, 22 (226, 31) decore] *Ia decora M*
 220, 30-31 (227, 8) in gloriam perseuerantia] *Ia in gloria per-*
seuerantiam M 221, 1 (227, 9) auctio] *Ia auctor M*
 221, 11, 227, 18 nihil] *Ia om. M*¹

Here are 38 readings of Aldus, adopted by Keil, though without warrant from *M*. In most of them, *M* is obviously in error. One might imagine that Aldus, starting with these errors, had emended them by conjecture. After Mr. Merrill's list of Aldus's deviations from *P*, the same features might seem to be displayed here — the fussy interpolation of little words (*sunt* and *et* and *iam*); the readiness to fill in lacunae even of considerable length (216, 14-15 = 222, 26-27); the abandonment of a satisfactory text presented by the ancient codex (219, 11 = 225, 22). If some such thought as this hovered before Keil's mind, he dismissed it when he wrote out his text, which follows Aldus in all of these cases. Damned authorities are sometimes used. And now appears *I*, verifying Aldus's integrity and Keil's critical intuitions.

I add, with proper hesitation, a few instances in which, though Keil followed Aldus, this confidence is not supported by *I*.

218, 31 (225, 7) et fregit et rapuit] *a Merrill fregit et rapuit MI*
 Here is the most difficult problem that I have encountered in the proper interpretation of Mr. Merrill's utterances. Both in his earlier and in his latest list, this case figures amongst the flagrant conjectures of Aldus. But in his Teubner edition, which appeared midway between the lists, Mr. Merrill joins the party of Aldus and Keil. Perhaps the reason is that either Mr. Merrill or his printer has in the apparatus wrongly attributed the first *et* to *I* as well as to *a*. But it is not in *I*. Therefore — *horresco referens* — Mr. Merrill has accepted

¹ Mr. Merrill reports *nihil* for *i* as well as for *Ia*. Just how *i* comes in I fail to see. He has no note or mark here of any kind.

a conjecture of Aldus's, not being prevented thereby from including it later in his renewed condemnation of Aldus. Here is one of those mysteries before which I retreat even as the ancient Virgilian commentators before the presence of "one of the thirteen."

219, 4 (225, 15) *loca*] *a om. MI, Merrill*. On this passage, see p. 176 below. 219, 32 (225, 33) *quoniam*] *a om. MI, Merrill*.

37 (226, 6) *prudential*] *a om. MI, Merrill*

In these places, Mr. Merrill doubtless thinks that Keil has yielded to the voice of the tempter. I will examine them all later from another point of view.

5. *Errors of Aldus also found in I*

I now would present another list, no less important for our problem than that which has just been given, illustrating the agreement of *a* and *I* in what Keil considers errors. It is a rather lengthy list. The readings that I mark with a star (*) I think should either be accepted or at least most carefully considered. Mr. Merrill agrees with Keil unless otherwise stated. In most cases, I have cited the second edition of Catanaeus.

210, 23 (217, 1) *quamquam*] *M tanquam Ia*

* 211, 9 (217, 10) *di*] *dei Ia dii M*

211, 14 (16) *praebent, praebent*] *M Cat.² praebent Ia*

Independent omission is easy, but *Ia* might have found one *praebent* already omitted in *P*.

212, 4 (218, 4) *praegnantibus*] *M praegnationibus a praegnationis I (in praegnationibus i, in praegnantibus marg. i)*.

Two varieties of *i* appear here. Mr. Merrill reports *praenantibus* for *i*² (in the margin), which would be an extraordinary correction indeed. But it is not there. In the text, the sign *./.* is written above the *a* of *praegnationis* (corrected by *i* to *praegnationibus*) and is repeated in the margin followed by *gnanb(us)* with a flourish above *na*; *i*² evidently intended *praegnantibus*. Catanaeus has *praegnationibus* in his text, but adds the note: *Legitur etiam praegnantibus*. He had doubtless seen it in the "*Codex Pontificius*," i.e. *M*. If *i* is correcting *praegnationis* to *praegnationibus* by *P*, he found there just what Aldus somehow has.

* 212, 4 (218, 8) *dis*] *diis MIa*

212, 9 (14) uideor a] *i* Cat.² uideo a *Ia* uideor *M*
Video may be construed with *iter* as its object, *et* — *nomina et* — *imagines* being the double object of *relicturus*, but *uideor* seems better and is attested by *M*. Note again that Catanaeus has secured the correct reading, probably with the help of *M*. And again the error of *I* is exactly the reading of *a*. What could *P* have had?

* 212, 21 (218, 26) metiri] *M* remetiri *Ia* Cat.², *Merrill*

* 212, 27 (219, 2) uenia] *M* uenia est *Ia* Cat.², *Merrill*

212, 30 (5) Titinius] Cat.² tittinius *Ia* tinius *M*

* 213, 5 (219, 12) in urbe] *M* in urbe est *Ia*

213, 6 (14) tererer] *M* Cat.² terrerer *Ia*

* 213, 7 (219, 15) obeunti uadimonia] *M* obeunti ad uadimonia
Ia Cat.²

See the passages collected by Catanaeus. *Obire* with the accusative is Ciceronian, but *ad* is used not infrequently by the later authors, beginning with *Livy*.

213, 12 (219, 20) materiae] *M*, (?) *i*, Cat.² materia *a*, (?) *I*

I report this reading as doubtful in *I*, since Mr. Merrill gives it as *materiae*, and yet it seems fairly certain that *I* wrote *materia* at first, which was changed either by the scribe or by *i* to *materiae* by the addition of a small semicircle to the right-hand shaft of *a* not quite at the top. The ligature resulting suggests a make-shift. Exactly the same kind of correction is made and reported by Mr. Merrill in 211, 1 (217, 2), where *i* used just such a semicircle to change *agerrime* (*I*) to *aegerrime*. This instance is included by Mr. Merrill, in his earlier list, among the wilful deviations of Aldus, but it is a strange "emendation" for anybody to make with the simple phrase *uel honestate materiae* before him. I believe, rather, that Aldus found *materia* in *P*, as the evidence of *I* seems to show, and that instead of venturing the obvious emendation which we find in *I*, he made the best of the difficult reading, taking *materia* as in parallel construction with *honestate* and putting a comma between them. In other words, Aldus acts here exactly as we should expect him to act from what we have seen of his tendencies. And even if we suppose that *materia* in *I* is merely a scribal error which *I* or *i* corrects from *P*, then the same scribal error is possible for *a*, unless we beg the question and regard all of Aldus's changes as wilful alterations of his only source.

* 213, 13 (219, 21) his] *M* iis *Ia Cat.*²

213, 14 (23) funebribus] *M Cat.*² funeribus *Ia*. Again Catanaeus has apparently consulted *M*.

? 213, 21 (219, 31) uiuum] *I. F. Gronovius* unum *MIa Cat.*²

214, 16 (220, 24) silenti] *M* silentii *Ia Bud., Cat.*²

* 214, 18 (220, 26) fidissimum] *M* fidelissimum *Ia Cat.*² Mr. Merrill has *fidissimum* in his text but *fidelissimum* stands first in his apparatus.

* 214, 18 (220, 26) praecipiendi] *uulgo* percipiendi *MIa Cat.*²

214, 31 (221, 10) quolibet] *M* quodlibet *Ia Cat.*²

214, 32 (11) scimus] *Reifferscheid* sumus *MIa Cat.*²

* 215, 10 (221, 21) alius (quis? inquis: ego)] *Cortius* alius inquis ego *M* alius quis ego *a Cat.*² alioquis (uel alius quis *margin.*) *I*

Aldus is confirmed by *I* in the omission of *inquis*, which looks like one of the characteristic glosses of *M*. Of course its omission is easy after *quis*, as is that of *quis* itself, so that both branches may have made different errors and Kortte be right. But the reading of *a*, which is that of *I* in the margin and hence, I take it, of *P*, is perfectly satisfactory and should, in my opinion, be accepted and thus printed: *Hos alius (Quis? Ego; sed nihil refert)*. The variant in *I* is discussed below, p. 175.

215, 13 (221, 25) non hercule] *Mi* at non hercule *Ia*.

* 216, 11 (222, 23) quia] *M Cat.*² quae *Ia Bud., Merrill*. *Quia* is neater but *quae* by no means impossible.

216, 23 (223, 2) consul] *M* consulere *Ia Cat.*²

* 216, 33 (223, 12) extiterit] *M* exierit *Ia Bud., Merrill* exegerit *Cat.*²

Catanaeus has in his note; "*Legitur etiam superior exierit uel extiterit: quod non improbo.*"

* 217, 5 (223, 16) discedant] *M* discedunt *Ia Cat.*²

? 217, 9-10 (20) an postulare debuerim, quem ad modum obtinui? qui] *M* an postulare debuerim. quemadmodum abstinuit is qui *I Bud.* an postulare debuerim, an abstinere, quemadmodum abstinuit is qui *a Cat.*²

This is a most difficult passage. Pliny is consulting a friend wise in the law and parliamentary procedure as to whether he acted rightly in a recent meeting of the Senate in causing three motions on a certain

case to be acted on separately. Pliny favored No. 1, for which there were evidently more votes than for either No. 2 or No. 3, but not more than for No. 2 and No. 3 together. He won his point, but now wonders whether he ought to have won it. "*Obtinui quidem quod postulabam; nihilo minus tamen quaero an postulare debuerim.*" "But to what extent did I win it?" This is the text of *M* — "*quem ad modum obtinui?*" Pliny goes on to explain that the advocate of No. 3 (*qui ultimum supplicium sumendum esse censebat*) joined forces with those who espoused No. 2 with the result that No. 2 was adopted. This makes sense, but so does the reading of *I* and Budaeus — or at least it contains sense. "*An postulare debuerim quemadmodum abstinuit is qui*" etc., — this implies that Pliny might have followed the example of No. 3 who withdrew his motion in favor of No. 2. Perhaps that should have been Pliny's procedure; the result would have been the same. This text seems to have satisfied Budaeus, no mean authority on matters constitutional, and with the addition of *an abstinere* inserted by Aldus after *debuerim*, was accepted by Catanaeus, the keenest Plinian critic of his day, although the reading of *M* was accessible to him and was doubtless studied by him, as he had devoted some thought to *extiterit* only eight lines above.

Now if *abstinuit* is to be accepted, *an abstinere* or something like it seems indispensable. "Ought I to have pressed my point just like the man who withdrew his" is condensation that borders on the absurd. It may well have been that the phrase inserted by Aldus was in *P* and carelessly omitted by *I*; we have noted an omission of this kind (p. 159) on the part of this scribe. It may seem strange that *M* should have made precisely the same omission when there are no similar endings to lead him astray, but this assumption, too, may become more plausible in view of certain considerations which I shall present below (p. 172). I will again crave the reader's patience until I have been fully heard. For the moment, let us suppose that Aldus has *suo Marte* interpolated the phrase *an abstinere* and that *obtinui* in *M* is correct. It is surely no good example of his disposition to "abandon indubitably satisfactory readings of his only and much belauded manuscript in favor of conjectures of his own." Mr. Merrill presents this case again in his latest list, but I submit that he does not present it fairly. When the reader sees only (p. 114): 223, 20 debuerim quem ad

modum] *MI* debuerim an abstinere quemadmodum *a*, he naturally agrees that by the concordant testimony of *MI* Aldus has been caught in the act of interpolating and infers that this is a splendid instance of a wilful deviation from a satisfactory reading in *P* which the witness of *MI* establishes. But the reader would not know unless he looked up the passage, and considered it as a whole, that in the real *crux* which it contains (obtinui *M* abstinuit *Ia*) *I* and *a* are involved in the same error. This error Aldus does not emend away, but retains. His interpolation is devoted to carrying out the idea contained, but illogically expressed, in the reading of *P*. Mr. Merrill asks (p. 114): "Would any critic except Mr. Rand think that in such cases as the following [this one among them] Aldus could have thought the readings of *P* 'obvious mistakes,' or could have been giving *P* the benefit of the doubt?" I would submit that when all the facts are brought out, the present passage is an admirable example of what I thought, and think, about Aldus. What does Mr. Merrill believe the reading of *P* here is? If *P* did not have *abstinuit* how did *I* get that reading? By a simple scribal error — resulting in the same word that in Aldus is a wilful conjecture? Does *I* try to emend? Hardly! *Abstinuit* was in *P*. The "obvious mistake" which Aldus found was the lack of some phrase to bridge over the meaning in this clause to that in the clause preceding. The conjecture to which he felt driven was the insertion of *an abstinere*. He "gave *P* the benefit of the doubt" in not trying to substitute some different meaning for *abstinuit*. When Mr. Merrill cries, "*Provoco ad populum*," he should favor his judges with the complete evidence in the case.

* 218, 6 (224, 15) quod] *M* cum *Ia* *Cat.*²

218, 8 (17) ut iussus] *M* otius (*in* ocus *i*), suis *I* ocus. suis *a* ocus. suis *Cat.*²

* 218, 28 (225, 4) sistere] *M* retro *Ia* *Cat.*² Merrill *Sistere* is one of the glosses of the *MV* family.

* 219, 5 (225, 16) deiectis] *M* deiecti *Ia* *Cat.*² Merrill

219, 10 (20) tale] *M* tale est *Ia* *Cat.*² Merrill¹

M has a habit of dropping little words. See the instances in the list on pp. 161 ff.

¹ Mr. Merrill besides reporting *tale est* for *Ia* attributes *tale* to *a*; this seems to be a slip for *M*.

* 219, 21 (225, 32) proneptem] *M* pronepotem *Ia Cat.*² *Merrill*

? 220, 1 (226, 8) Curtilius] *Casaubon* curtius *MIa Cat.*²

Catanaeus cites the passage in Tacitus, *Annales* 13, 56 before Casaubon did, but he prefers to emend Tacitus by Pliny and not vice versa. Perhaps this is the better way, seeing that our text of Tacitus derives from a single manuscript, whereas in Pliny the form Curtilius is supported by both Class I and Class II. An instance like this shows the importance of determining the value of the different classes and in particular of ascertaining whether they descend from antiquity in one line or in two independent lines.

220, 1 (226, 8) Mancia] *M* Mantia *Cat.*² Mancipia *Ia*. In *I*, the sign ÷ is written above *mancipia* and repeated in the margin, although no variant is added there. Perhaps the intention was to put in that of *M*. See below, p. 177.

* 220, 2 (226, 9) is] *M* est *Ia Cat.*² *Merrill*

220, 5 (226, 12) fratris] *M* patris *Ia* potestatem] (potestatem (*corr.* ex poni statem) *M* potestate *a* (*in* potestatem *i*) *I*

220, 9 (226, 16) nomen] *Mi* numen *Ia*

* 220, 9 (226, 17) decem et octo] decim et octo *M* octo et decem *Ia Cat.*² *Merrill*

* 220, 27 (227, 4) auditum] *M* auditum est *Ia Cat.*² *Merrill*. Again *M* drops a small word. * 220, 28 (227, 4) ex] *M* ab *Ia Cat.*² *Merrill*

221, 4 (227, 12) neglegebat] neglegebat *M* neglegebatur *a* neclegeba(n)tur (neglegebat(ur) *marg.*) *I* negligebantur *Cat.*² negligebantur *Merrill*

The reading of *I* is exhibited on our Plate XVIIb. The stroke connecting *nec* and *legeba(n)tur* is probably but not surely by the first hand. The stroke over *a* is not like that generally employed by *I* as a compendium; he uses a long, fine sweeping stroke. The variants of *I* are *nec legebatur* in the text and *neglegebatur* in the margin; the scribe, or perhaps *i*, later connecting the two words in the text. It seems surely *i* that changes the singular to the plural form. The concordance of *I* and *a* seems to establish the passive form for *P*, but the plural may be a conjecture received by *i* from Catanaeus. It is a good one and should be accepted. It is of course possible that *P* was correct, *I* and Aldus making easy independent errors. I had written this note before Mr. Merrill's edition appeared and was glad to find that, on whatever grounds, he reads *neglegebantur*.

221, 5 (227, 13) quid istic epistula] *M Cat.*² quid epistola (*ex* quid de epistola *I*) *Ia*. *Istic* seems too good for a gloss. Catanaeus has again consulted *M*.

* 221, 13 (227, 20) confugi] *Mr.* confugio *Ia Cat.*²

221, 17 (25) ipse] *M* ipsi *Ia*

Here are 47 readings,¹ in which the credit of Aldus — very shaky, it would seem, as one goes through Keil's apparatus — is now supported by the testimony of *I*. Opinions will differ. I believe, against Keil, whose judgments are natural enough on the basis of the material accessible to him, that some 22 of these readings, and possibly a few more, deserve a permanent place in the text. Of those that I have starred, Mr. Merrill accepts 12, hard though it must have been for him to conclude that Keil *versus* Aldus was wrong. My list was made up before Mr. Merrill's edition appeared; it is gratifying to have at least this partial confirmation from him. At all events, from 15 to 20 cases there are which no editor to-day would hesitate to call certain errors. Some of these readings might perhaps be swallowed, but they are by no means toothsome morsels. Now as they are all supported by *I*, either

(1) they were not in *P* but were independent scribal errors on the part of *I* and *a*, or

(2) they were not in *P* but are scribal errors in *I* and wilful "emendations" on the part of Aldus, or

(3) they were not in *P* but are scribal errors in *a* and wilful "emendations" in *I*, or

(4) they were in *P* and retained without emendation by *I* and *a*. The character and the number of the readings exclude, in my opinion, the first hypothesis. I hardly see how Mr. Merrill or anybody else can favor the second; it involves the assumption that in at least fifteen cases Aldus suspected his venerable codex of error (though it was right all the time), and cracking his brains for conjectures, happened to hit in each and every attempt on nothing better, and nothing

¹ From Mr. Merrill's apparatus it would seem possible to make one addition to this list. 215, 1 (221, 11) errori siquis] *M* erroris siquis *Ia*. But *I* plainly has *errori*. If Mr. Merrill's statement had been correct, I suppose we might have inferred that Aldus and *I* had independently committed an easy mistake. But now that Aldus is left alone in error, shall we not explain his reading as a wilful conjecture?

else, than the mistake committed, unknown to him, by *I*. No. 3, I can state with all positiveness would never be accepted by Mr. Merrill or anybody else. I think, therefore, that Mr. Merrill might join me in approving the fourth hypothesis, were it not that it lends support to my statement, which elicited Mr. Merrill's astonishment, that Aldus is inclined to follow even the errors of *Parisinus*, unless they are patently impossible — what I called its "obvious mistakes." Since he did not have *M* at his disposal, as Catanaeus did, we cannot tell what he would have done with the readings of that manuscript. But at least here is a goodly number of passages in which we now know that the text of *P* was inferior but which did not stir Aldus to conjectural emendation. Which, if any, of the four hypotheses Mr. Merrill does accept, I shall be interested to know. Perhaps there is some other one of which I could not think.

At all events, we have significant testimony here, which should be weighed with that which Mr. Merrill has presented on the other side. Really, he condemns Aldus on half of the evidence. The discovery of *I* enables him to compile a list of at least nineteen cases — I say "at least" because his latest list contains "samples only" — in which he thinks that Aldus has wilfully and unnecessarily deviated from his ancient source. But the discovery of *I* also enables us to test Aldus in eighty-five important readings and to find that in all of these, some of them right and some of them wrong, Aldus agrees with *I* against *M* and therefore, presumably, is, no less than *I*, faithfully reproducing the text of *P*. What system of academic rating was ever so merciless as to mark a poor chap only on his mistakes and never on his happy passes?

6. *The original of I probably a minuscule manuscript*

We are not done with our examination of *I*. In the Carnegie publication, I suggested (p. 62), on the basis of our photograph of one page (see Plate XVIIb), that the scribe was copying not a majuscule but a minuscule original. That surmise was strengthened when I examined the rotographs of the other pages. Here are certain errors of *I* to consider.

211, 5 (217, 6) uiridi] uitidi (t *del.* r ss.) *I*

211, 8 (217, 9) indicant] iudicant (u *del.* n ss.) *I*

211, 30 (218, 4) audies] indies (in *del. au ss. i*) *I*

212, 24 (218, 30) solari] *I* evidently first wrote *solori*, the *o* being later made into an *a* by the addition of a shaft at the right. It is utterly unlike the usual *a* of the scribe, and for the sake of clearness a small *a* was added above the line. This was somewhat blotted, and hence another of the same kind was put in the margin, both these additions apparently being the work of *i*. Who corrected the *o* in the text it were rash to say. The point of concern to us is that the mistaking of an *a* for an *o* would hardly be expected of a scribe copying majuscule.

213, 2 (219, 9) denique] diemq(ue) (*corr. i ut uidetur*) *I*

213, 22 (219, 31) coniunctissimum] couinctissimu (m) (*corr. i ut uidetur*) *I*

215, 10 (221, 18) alius quis] alioquis (u(e)l alius q(ui)s *marg. u(e)l postea del.*) *I*

Alioquis seems to presuppose a minuscule abbreviation (*ali' quis*). On this reading see above p. 165 and below p. 175.

217, 20 (223, 31) uinceretur] uincetur (re *ss. i*) *I*

The omission of a syllable is an easy affair in the copying of any script, but especially so if it was expressed with a minuscule abbreviation.

218, 29 (225, 5) Anio] Auio (*corr. i*) *I*

219, 17 (28) morum] mortis (*corr. i*) *I* 221, 4 (227, 12) quae] quasi *I*
This is a curious error, left uncorrected in *I*. It would seem as if the compendia for *quae* and *quasi* were somewhat similar in the manuscript copied by *I*. If so, the most probable script would be a late minuscule.

221, 6 (227, 14) tum] tamen (*del. et tum marg. i*) *I*

This is the instance to which *I* referred in our article (p. 62, Plate XVIIb). The error is surely most easily explained if we assume that the scribe mistook a minuscule compendium.

This array of readings indicates pretty clearly — *I* do not assert that it absolutely proves — that the original which lay before *I* was a manuscript in minuscule of not too early date. If the Morgan fragment was part of *P*, the scribe was not copying that manuscript.

But not to keep secret any possible evidence on this matter, *I* will admit that certain readings suggest that the scribe was puzzled with the proper division of words, as though his original were in *scriptura continua*.

213, 4 (219, 11) benignitate mira] benignitatem / ira *I corr. i.*

214, 9 (220, 16) adulescentuli statim] adulescentulis statim (*corr. i?*) *I*

215, 27 (222, 5) morte multaret] mortem uitaret *I* (*m et uitaret del. multaret marg.*) *i*

The first two examples might have been due to false hearing and mental confusion occurring when the scribe dictated the words to himself. But that is not true in the present case. The scribe must have *looked* at the text to disconnect the *m* from *multaret* and to mistake *l* for *i*. This error could have most easily been made in copying rustic capitals or a sort of minuscules in which either *i longa* was used or in which *l* did not much protrude above the line. The spelling used by *i* is worth noting. It doubtless was not that of *P*. Either Budaeus makes the change in the act of collating, or, as would seem more likely, takes *multaret* from *Cat.*², who had it in this form.

215, 28 (222, 6) diuidi sententiam] diuidissent iam iam (*corr. i*) *I*
On this error see below, p. 175.

218, 19 (224, 28) amici sinu] amicissii sinu *I*. See above, p. 160.

219, 3-4 (225, 14) uarie lateque] uarietateque (*corr. marg. I*)

After all, this is not an imposing array. Some of these confusions may well go back to *P*, which according to my theory, was an uncial manuscript. Were *I* a direct copy of such a manuscript, we should expect more such cases. If a copy intervened between *I* and *P*, errors of this sort would in most instances have been cleared up.

The total impression that I gather from the errors of *I* is that its original was written in minuscules. Perhaps Mr. Merrill would not object, especially if he still holds it possible that Aldus's much be-lauded *Parisinus* was a Gothic codex of the twelfth or thirteenth century.

7. The original of *I* an *M*-text collated with *P*

But there is a final consideration that leads us not to a mere opinion but to the certainty that the original of *I*, although deriving most of its strength from *P*, was *not that manuscript itself but a contemporary and conflated copy*. This state of affairs I had little suspected from the account of the Bodleian volume given by Mr. Merrill or his vigorous presentation of his estimate of that book. The material for forming

an estimate is now accessible in his edition, and the appearance of a page of *I*, with marginal corrections by both *I* and *i*, is illustrated in our Plate XVIIb.

(a) *Conflated readings in I*

My suspicions were first aroused by the condition of *I* in the following passage.

218, 33 (225, 10) *euexit*] *a* *ieicit* *M*

Here is one of the few readings of Aldus that Keil preferred to those of *M*. Neither word is objectionable in itself, but supposing that Aldus is drawing from *P* here, we naturally decide for his reading, knowing as we now do that it is the *M* line of the tradition that is infected with glosses rather than the family which *BF* and Aldus represent. Catanaeus, though not ill disposed to *euexit*, proposed *erexit*.¹ He must have known the reading of *M*, for he had collated that manuscript on this passage, as we shall see in a moment. For reasons best known to himself, he abandoned both *euexit* and *ieicit* for his own conjecture. Turning now to *I*, we find in the text *ieicit*, with the sign ÷ written above, and in the left margin, under the same sign, *euexit*. What are we to make of this? Was there after all, a doublet in *P*? We have noted the workings of a second hand in the Morgan fragment, which "emended" *sera* to *serua*, supplied a missing title, and introduced several other changes, some of them rightly (p. 113). Possibly, then, in *P* *ieicit* was written above *euexit* — in which case *I* preferred the gloss to the text — or *euexit* was written above *ieicit* — in which Aldus preferred gloss to text. Still, in the text comprised in II (I am continuing to assume that this fragment was a part of *P*) Aldus took the second hand only when forced to it; he did not take *serua* for *sera*. Supposing, now, that *ieicit* was not in *P*, did *I* invent it as he wrote? This is most unlikely, from what we have seen of the habits of this scribe. It would also be unlikely that he should happen to invent the very reading of *M*. How, by the way, did Aldus get *euexit*? The readiest answer, in accordance with Mr. Merrill's principle, is to say that he made it up; it is one of his wilful conjectures. But if so, how did he happen to hit by conjecture on a marginal reading of *I*'s? Once more, there might have been a double reading

¹ *Erexit*] *excreuit*, *extulit*, *transmisit*; *non improbo euexit*.

in *P*, but another possibility is that *P* read *euexit*, which was written above *ieicit* in the text that was copied by *I*. That text, therefore, was of the *M* variety, corrected from *P*. I hasten to add that this is one of two possible assumptions and not yet proved. But the following reading, unless my reasoning faculties are utterly perverted, furnishes clear proof of the conflated condition of the text of *I*.

219, 3 (225, 14) *atque culmina*] *a om. M* ("*codex pontificius non habet atque culmina*") *Cat.*²

This is in the same passage as that just discussed, a description of a freshet. The entire sentence is:

Viderunt quos excelsioribus terris illa tempestas deprehendit (non deprehendit *a*) alibi divitum apparatus et grauem supellectilem, alibi instrumenta ruris, ibi boues aratra rectores, hic soluta et libera armenta atque inter haec arborum truncos aut uillarum trabes *atque culmina* uarie lateque fluitantia.

Here, once more, what a hostile critic might adjudge one of Aldus's deliberate inventions seemed to Keil an appropriate adornment of the sentence (at least Keil has it in his text); *fluitantia* can be construed without the added phrase, but is neater and smoother with it.

And now for *I*. The scribe in his text has made the error of *uarietateq(ue)* for *uarie lateque*, and has omitted *atque culmina*. He puts the sign ÷ (used, as we have seen in the *ieicit-euexit* reading) above *uarietateq(ue)* and in the left margin after the same sign writes *atq(ue) culmina uarie lateq(ue)*.

This seems to me a crucial case. With regard to the intrinsic merits of *ieicit* and *euexit* one might ponder long, but here is a fragment of a sentence too good, it strikes me, for a gloss or conjecture. If the words are genuine, it is most unlikely that both *M* and the scribe of *I* should independently omit them — there are no similar endings to mislead — though any one scribe could make such an omission, as *M* not infrequently does.¹ If the words are not genuine, how is it that the wilful conjecture of Aldus happens to coincide with the marginal reading of *I*? Is it a conjecture there? If so, how could Aldus have found it out? If *i* had made the addition, we should naturally call it a borrowing from either Catanaeus or Aldus himself; but the hand is clearly that of *I*. The conclusion is (to me) inevitable that *atque*

¹ See, for instance, the list on p. 161 above.

culmina was found in *P*. It was copied by Aldus and by the scribe (I will call him *I*^o) who wrote the original of *I*, that is, *I*^o, after writing out some *M*-text, inserted *atque culmina*, either in the margin or between the lines, along with the other variants from *P*. As these variants must have been numerous, it is not surprising if here and elsewhere *I* failed to get them all. But he got this one eventually and put it in the margin. He thereby betrays the conflated nature of the text that he copies.

In the light of this conclusion, we may now consider certain other passages, some of which we have partly discussed already.

213, 4 (219, 11-12) *benignitate mira] a benignitatem ira M benignitatem / ira (corr. i) I*

We have looked at this case before as a possible bit of evidence that *I* copied a text in *scriptura continua*. We should now add that if his original was an *M*-text corrected from *P*, the mistake might have already been in that original.

215, 10 (221, 21) *alius quis]*

For the readings, see above, pp. 165, 171. *Inquis*, I concluded, might well be one of the glosses of *M*. In *I*^o, it might still have been above the line, and not as in *M*, incorporated in the text, and thus could have been passed over by *I*. *Alius quis*, I have also surmised, was written in the text with a minuscule abbreviation. This would account for *alioquis* in *I*. But this abbreviation must now be pushed at least one stage farther back. The error *alioquis* was already in the text of *I*^o. If *I*^o had had *ali(us) quis*, with *us* expressed by a suprascript semicircle which *I* mistook for *o* and later discovered the mistake, the scribe could readily have emended his blunder by changing *o* to *us* between the lines; this is how he corrects *uitidi* to *uiridi* (211, 5 = 217, 6). But here he uses a reference sign to the margin and there adds not a correction, but a variant — *v(e)l alius q(ui)s*. Corrections are not generally introduced by *uel*. But *i* (apparently it is *i*) changed the variant to a correction by deleting the *vel*. *I*^o, therefore, copied *alioquis* from its *M*-source and added *alius quis* as a variant from *P*.

215, 28 (222, 6) *diuidi sententiam] diuidissent iam iam (corr. i) I*. This error has had something of a history. If *I* is copying an original in *scriptura continua*, he not only makes a false division of words (*diuidisent entiam*) but “emends” by inserting an *s* and then by a

similarly desperate effort changes *entiam* to *iam iam*. So much alteration does not comport with the conservative habits of the scribe *I*. He is copying not *P* directly but an intermediate copy which has been subjected to a considerable amount of revision. In the present case we have an emendation that is both stupid and audacious.

218, 31 (225, 7) et fregit et rapuit] *a* fregit et rapuit *MI*

This case may be thus explained. *I*^o, if he here collates his *M*-text carefully with *P*, writes *et* above the line before *fregit*, but *I* fails to take it.

219, 4 (225, 15) illa quidem loca . . . ad quae] *a* *Cat.*² illa quidem . . . quae *M* (ad marg.) *I* illa quidem . . . ad quae *Merrill*

This is an instructive passage, which I would explain thus. Pliny in describing the freshet speaks of the injury suffered even by the places to which the floods did not ascend — *ac ne illa quidem loca malo uacauerunt, ad quae non ascendit amnis*. Such is the text of Aldus and of Keil. Mr. Merrill, naturally applying his principle, infers, as Kortte had done before him, that *loca* is an interpolation of Aldus's. But how about *ad*? It can hardly be said that *ad*, clearly needed for the sentence, has been independently omitted by both *M* and *I* and then replaced by *I*. The procedure of *I* is too deliberate for that. Before *quae* he adds a cross (+); the same sign stands in the margin and under it *v(e)l ad*. The scribe would not preface *uel* to a word that he knew he had just omitted. He is copying a text like *M* that did not contain *ad*; he is using *uel* as he did for the variant *alius quis* (p. 115). The word had been added by *I*^o when he collated *P*. He may also have written *loca* between the lines, *I* failing to observe it. The whole passage would have presented a botched appearance to the scribe; immediately above, there was the variant *atque culmina varie lateq(ue)* to be written in the margin. It is not surprising if he made some omission.

219, 26 (226, 5) quos sic decipere pro moribus temporum prudentia est] *a* *Cat.*² *Cortius* quod sic pro moribus temporum est *M* quos sic decipit (*corr. in decipi*) *I* quos sic decipi pro moribus temporum est *Merrill*

I agrees with *M* in omitting *prudentia* but not in omitting *decipere* (*decipi*). *M*, as we have noted several times, is rather prone to the omission of words or short phrases; after *decipere* had been thus

omitted, and *prudencia* too, either *M* or some ancestor changed *quos* to *quod* in a vain attempt to make some sense of the remainder. Of course when Aldus suddenly flaunts *decipere* and *prudencia* before us, we know that he has been indulging in his habit of conjecture. It is not such a bad conjecture; it is accepted by Keil and even by Kortte, the most vigorous critic of the Aldine text until Mr. Merrill published. Longolius in editing Kortte's notes quotes the defective reading of *M* and adds, "*Cet. reliqua omittit; nescio pietate librarii an alia de caussa.*" It is not, I think, a case of scribal *pietas*. As for Aldus, we find him at least half-authenticated by *I* — that is unless we regard *I*'s *decipi(t)* as an independent conjecture. But emendations in *I*! The matter is cleared up, it seems to me, if we suppose that *I*^o wrote above the lines the words missing in his *M*-text. If *decipere* was abbreviated he might have made *decipit* out of it, leading to the natural correction, whether by *I* or *i* is hard to say, into *decipi*. But *I* failed to add *prudencia*.

The meaning of the Aldine text is plain enough. In Melmoth's translation it runs: "This infamous tribe of men, whom, considering the manner of the age, it is but prudence to deceive in this way." What is the sense of the reading of (the emended) *I* adopted by Mr. Merrill, I wish he would explain. If *pro* means "in accordance with," "in conformity to," Pliny is made to say that the deception of the *captator* was all the vogue. I had somehow thought that villains of the stripe of Regulus not infrequently escaped deception. If *pro* means "in behalf of," "for the advantage of," there is a meaning in the phrase, though less pungent than that in Aldus's sentence, but *esse pro aliqua re* instead of *prodesse alicui rei* seems to me curious if not dubious as a Latin construction. Perhaps there is some elucidation that I have failed to fathom. Mr. Merrill and Titze are the only editors, so far as I am aware, who have dispensed with *prudencia*. I think that with Keil and Kortte we should accept the Aldine reading and not mutilate either the sense or the Latinity of Pliny's epigram. Aldus found it in *P*. The original of *I* had partly corrected his *M*-text, but not thoroughly.

220, 1 (226, 8) Mancian] *M* mancipia a (nota ÷ suprascripta et in marg. repetita) *I*

Here the *Parisinus* reading, incorrect though it was, was put in the

text by *I*, but he apparently knew of some variant which he meant to add and then, whether by neglect or by intention, omitted. It would seem as though this variant was *mancia*, read correctly by *M*. However, as *i* also uses this sign, it may be that Budaeus, dissatisfied with *mancipia*, meant to insert a variant or conjecture; *Mancia* was accessible to him in Catanaeus's second edition, but perhaps the signs were put in before that time. We had better leave this passage with a *non liquet*, though as Mr. Merrill, who studied the Bodleian volume with care, ascribes the signs to *I*, the case at least deserves consideration.

221, 4 (227, 12) *neglegebat*] On the readings see above, p. 168. It will now be evident that the marginal variant, *neglegebatur*, should, according to the theory that I am presenting, be the reading of *P*. It is wrong, and Aldus has it; he is following, all too faithfully, his ancient source.

(b) *Collapse of Mr. Merrill's criterion*

It thus appears that *I* is a conflated text. This condition is proved, in my estimation, by the *atque culmina*, *ad* and *alius quis* readings. The assumption of such a text best explains the other readings which I have just discussed, and there is no reading throughout the text that we are considering which cannot be thus explained. The basis of the text *I*^o, then, is a manuscript like *M*, which, though perhaps free from some of the errors of that manuscript, probably had others of its own; this copy was thoroughly revised from *P*. In this process, most of the imperfections of *M* disappear, but others are introduced from the other source. For in collating *P*, the scribe of *I*^o did not invariably get correct readings. How could he be sure? He was impressed with the value of the ancient book, and may well have wished, in the manner of Politian, to extract all or nearly all its variants. Some readings he probably did not get, and some that he did get were omitted by *I*. The result in *I* is a text which, substantially like that of *P*, has still some of the earmarks of *M*. It is a welcome source of information with regard to *P*, but the concordance of *M* and *I* can no longer be held to establish without further consideration the reading of *P*, for the reading in question may be merely an original *M*-reading that the scribe of *I* failed to change for that of *P*, which Aldus has preserved. Therewith Mr. Merrill's simple principle falls to the ground, and with it his weightiest indictment against Aldus.

(c) *The M-tradition ultimately French*

I fear that Mr. Merrill will aim another exclamation-point at my suggestion that a copy of *M* or other manuscript of its class could have been known to the scribe who, by my theory, prepared the original of *I* somewhere near Paris. For *M* represents what Mr. Merrill entitles the German tradition of the text of the *Letters*.¹ But as I showed in our publication (pp. 44 ff.) this so-called German tradition is, like most things Mediæval, ultimately French. *M* was written at Korvey, but *V* is a thoroughly French book, at least in my estimation. Mr. Merrill declares that Keil errs in thinking that it might possibly be earlier than *M*,² but it is likely that Keil was perfectly right. I have nothing to add to Ullman's careful demonstration that *V* is a book of Corbie,³ though still being inclined to think that it was written in the neighborhood of Fleury. This point is of no consequence at the moment. Let us say that *V* was written at Corbie or copied near Fleury from a manuscript of the former monastery. As it is so closely related to *M*, written at Korvey, the daughter-monastery of Corbie, it is highly probable that the source of both books is a codex of Corbie. The Leyden fragment (*Vossianus Latinus* 98, s. IX) is also French, and suggests the script of Fleury. Fleury and Corbie are the two most probable centres of provenience for this class of manuscripts. It is by no means impossible, therefore, that a copy of the parent manuscript of *MV* was obtained by Budaëus, or Jucundus, and collated with *P*.

Possibly, also, the complete manuscript of this family whence springs the mutilated text of the *Cusanus* and the *Pragensis* was accessible.⁴ These codices contain in nearly complete form the letters of Books 1-7, with an eighth book made up of letters from Books 7, 8 and 9. Those of the eighth book include the 14th and 18th, that is, an important stretch of text of that part of the eighth book which was

¹ *C. P.*, X, pp. 21 ff.: "The scholastically revised text that dates from the fifth or sixth century apparently migrated bodily in early days to Germany, where it played a prominent part, as *F* did in Italy." This doctrine is repeated in the Teubner edition, pp. XVI ff.

² *Op. cit.*, p. 24.

⁴ *C. P.*, XIV, pp. 24 ff.

³ *Philological Quarterly*, I (1922), pp. 17 ff.

printed by Aldus for the first time. There is no doubt of the German lineage of Nicolaus Cusanus, the owner of the first of these codices, or of the German origin of most of his manuscripts that can be traced to-day.¹ However, the *Pragensis*, written in Bohemia, is ascribed to the fourteenth century. From Mr. Merrill's description it is clear that *c* is not derived from *p*; accordingly they come from a common source, some extract-manuscript of the *Letters*, copied in the fourteenth century or earlier. The text of *p*, which I know only from Titze's edition,² is obviously of the same variety as that of *M*, as the appended readings, distinctly ascribed by Titze to *p*, are sufficient to show.³ The suspicious character of the text is proved by certain obvious and flagrant attempts to emend the errors found in *M*.⁴ At the same time, if I may trust Titze's text as representing *p* unless he states the contrary, there are some places in which *p* has a better reading than that of *M*.⁵ I give the list, purposely incomplete, with hesitation, since the scholar who was responsible for the unscrupulous alterations that we have noted might have obtained these good readings by conjecture. Still, the

¹ On Nicolaus Cusanus, see Sabbadini, *Scoperle*, 1914, pp. 16 ff.

² *Caii Plinii Caecilii Secundi Epistolarum Libri, ad Fidem maxime codicis praestantissimi Pragensis . . . recensuit . . . Franciscus Nicolaus Titze*, Lipsiae, 1823. Titze, who as Mr. Merrill showed (*C. P.*, XIV, pp. 24 ff.) grossly exaggerated the value of this codex, appears at least to have collated it carefully.

³ 214, 13 (220, 21) erat] *om. Mp*; 20 (28) suspecta] inspecta *Mp*; 215, 2 (221, 13) curae] cura *Mp*; 10 (26) quamquam] quam *Mp*; 10 (27) sententiae] sententia *Mp*; 216, 14-15 (222, 26-27) in hanc . . . sentitis] *om. Mp*; 20 (32) dubium] durum *Mp*; 217, 1 (223, 12) comprobata] con(m)parata *Mp*; 14 (24) absoluendos] soluendos *Mp*; 219, 22 (225, 33) inexpectata] ex(s)pectata *Mp*; 26 (226, 5) quod sic pro moribus temporum est *Mp*; 28 (7) filiae] ille *Mp*; 5 (12) fratris] patris *Mp*; 6 (13) et quidem cum opibus amplissimis] *om. Mp*; 10 (18) ut patres . . . curauerint *Mp*; 27 (227, 4) est] *om. Mp*; 28 (4) ab] ex *Mp*; 221, 2(9) auctio] auctor *Mp*.

⁴ 214, 7 (220, 13) autem] ante *M om. p*; 215, 8 (221, 19) an didiceris] adridiceris *M om. p*; 215, 10 (221, 21) quis . . . refert] *om. p*; 216, 24 (223, 3) solemnibus] lenibus *M lenibus p*; 220, 22 (226, 31) decore] decora *M decorum p*; 30 (227, 8) in gloriam perseuerantia] in gloria perseuerantiam *M in gloriam perseuerantiamque p*. In a word, when *M* has a reading egregiously wrong, *p* either omits it or indulges in free, and unsuccessful, emendation.

⁵ 215, 15 (221, 27) relegantis] *p (ut uidetur) religatis M*; 216, 3 (222, 14) periment] *p (ut uidetur) perimant M*; 11 (23) quae] *p (ut uidetur) quia M*; 217, 13 (223, 23) ne si] *p (ut uidetur) nisi M*.

errors of *M* thus emended might have eluded the observation of the critic, for in general they give a satisfactory sense. Perhaps Mr. Merrill can prove from his collations that the parent manuscript of *p* and *c* was derived directly from *M*. In this case, this mutilated copy of the nine-book family (Class II) would most probably have been written in Germany. Otherwise there is no certainty as to its provenience.¹

It is not incumbent on me to lay my hands on the very codex of Class II in which *I*^o inserted variants from *P*, but the possibility of its existence is not contradicted, as perhaps one might suppose, from Mr. Merrill's account of the history of the text of the *Letters*. Its actual existence, as I have sought to show, is demonstrated by the conflated character of the text of *I*.²

8. Aldus's "conjectures" re-examined

Let us return to the list of Aldus's supposed offences, the cases that I left³ as an apparently irreducible minimum of conjecture. With some of these I have already dealt. I will revert to those in which there is some ground for charging Aldus with resorting to conjecture, and will add the cases that remain in Mr. Merrill's earlier and longer list. I will treat these passages in the light of what we have seen of the nature of the text of *I*, marking with a star those in which I think that the Aldine reading should be restored.

* 214, 2 (220, 9) ignorantiam] *MI* ignorationem *a*

Kortte (Longolius) remarks: "*Utrumque a Cicerone adhiberi notum est. Hic cur a membranis* [he means *M*] *recedamus caussa nulla est*. This is sound utterance, with the spirit of which I think Aldus would have agreed. If he had found *ignorantiam* in *P*, he would not have been tempted to change it. There are three occurrences of *ignorantia* in Book 10,⁴ no one of which Aldus sees fit to disturb. What he found

¹ It may be worth noting that in 213, 26 (220, 4) *senatorium*, *p* and *I* have the same error, *senatorum*. That might have been the reading of the *M*-text followed by *I*^o. But independent error is of course easy.

² It is well to remember that Mr. Hardy (*Journal of Philology*, XVII, p. 95, regarded the manuscript part of the Bodleian volume as "having been copied either from the lost *Parisinus* Codex or more probably from a copy of that codex." He attributed the additions of *i* to Jucundus, following the opinion of Mr. Madan and Mr. Macray of the Bodleian that the hand was Italian (pp. 98, 104).

³ See above, pp. 158 ff. ⁴ 266, 1 (272, 4); 280, 33 (286, 18); 300, 14 (307, 5).

in *P*, I infer, was *ignorationem*. *Ignorantiam* was read by *M* and the *M*-text used by I°. If one wishes to decide Pliny's usage on the basis of arithmetical preponderance, *ignorantiam* is correct. If one wishes to allow Pliny a certain amount of variation, within Ciceronian limits, we should read *ignorationem*.

216, 3 (222, 14) periment] See above, p. 157. Here I think that Aldus adopted a conjecture, for the reason that *P* was in obvious error.

217, 9-10 (223, 20) an postulare debuerim etc.] See above, pp. 165 ff. Here the interpolation of Aldus (*an abstinere*) seems indispensable. It is not a flagrant desertion of the satisfactory reading of *P*, as one will see if the entire context is examined.

* 217, 27 (224, 8) quae si scabrae bibulaeue sint, aut non scribendum] *a om. MI*

Pliny has promised his friend that he will write him frequently if only he can procure the paper — *sit modo unde chartae emi possint*. He adds, in the text adopted by Mr. Merrill, *aut necessario quidquid scripserimus boni maliue delebimus*. As Keil explained in his note — in his text he accepts the reading of Aldus — Pliny may mean that he intends to erase his extant works, good and bad, to get writing material; his friend will receive a palimpsest correspondence. According to Aldus's text, Pliny is thinking of the kind of paper that he may be able to obtain. It was hard to get and poor if you got it; if it is rough and porous, why then he had better not write at all, or else whatever is written, good or bad, has been blotted out in the writing.¹ I find small reason for preferring either of these pale witticisms to the other. That of the *MI*-text, further, is obscurely enough expressed to warrant the suspicion that something has fallen out of the text. Now supposing for the moment that the *a*-text is acceptable, one word in it is wrong, *deleuimus*. Though *M* has a scribal error here (*mali uidebimus*), it has preserved the future form. The point of great interest is that not only *a* but *I* has *deleuimus*, the word being corrected in *I* by *i* to *delebimus*. *I* and *a* preserve the error of *P*, while *i* successfully emends. A far more daring, and correspondingly less successful, conjecture by *i* is *scripseris* for *scripserimus*. Here is a third witticism. If Pliny can

¹ "For should I be reduced to the coarse and spongy sort, either I must not write at all, or whatever I compose, whether good or bad, must necessarily undergo one cruel blot" (Melmoth).

get no paper, he will convert not his own but his friend's productions into palimpsest. This jest is the best of the three, but I am afraid that its author is Budaeus and not Pliny. With the reading of Budaeus, the banter is similar to that of Cicero at the expense of his economical correspondent Trebatius;¹ the talk there is plainly of palimpsests. As Kortte (Longolius) well observes, Pliny may have had this letter in mind. But he need not have said just the same thing. The reading in Kortte's text is the Aldine, or "vulgate" as it had become, and the following note is appended:

Mediceus: *boni mali videbimus*, corrupte. Is etiam ista: *quae si scabrae bibulae sint, aut non scribendum*, omittit. Puto interim veram esse scripturam vulgatam.

This seems to me the right decision. Pliny's wit is generally of a quiet order. Why make him sparkle? And he sparkles only dimly in the reading of *MI*. The obscurity of that reading, once more, suggests an omission of some phrase — a fault only too frequent in *M*. And though the present omission is somewhat long and not occasioned by similar endings, two successive errors might easily have been committed of precisely this nature. One scribe could have left out *quae si scabrae bibulae sint* because of the preceding *possint* and another have dropped *aut non scribendum* because of the following *aut*. It is not inconceivable that a single scribe should have made both omissions if he was copying a majuscule script word for word rather than phrase for phrase.

* 218, 31 (225, 7) et fregit et rapuit] See above, p. 162.

* 218, 33 (225, 10) uiderunt] *MI* uiderunt hi *a*

* 219, 1 (225, 11) deprehendit] *MI* non deprehendit *a*

The last three readings are from the description of the freshet. The context of the last two is, as Keil has it:

uiderunt quos excelsioribus terris illa tempestas deprehendit alibi diuitum apparatus et grauem supellectilem, alibi instrumenta ruris, ibi boues aratra rectores . . . atque inter haec arborum truncos aut uillarum trabes atque culmina uarie lateque fluitantia. ac ne illa quidem loca malo uacauerunt, ad quae non ascendit amnis. nam pro amne imber adsiduus, etc.

We have examined the latter part of this passage before (pp. 174 ff.) and approved Keil's retention of the text of Aldus there. He and Mr.

¹ *Ep. Fam.* 7, 18, 2.

Merrill do not accept Aldus's addition of *hi* and *non* in the first part of the sentence. I would read *hi* partly because it is not Aldus's habit to interpolate minor words of this sort: the list on pp. 161 ff. shows a number of what might have seemed such interpolations in *a* — but though they are not in *M* they are confirmed by the testimony of *I*. *Hi*, moreover, is in itself effective and dramatic. The sudden addition of *non* may well arouse suspicion. Kortte's defence of the reading of his "*Optimus codicum*" (*M*) has doubtless seemed conclusive to many:¹ "*Illud non deprehendit nihil est, et n. demum 5 (i. e. § 5) de iis locis, ad quae amnis non adscendit, dicere incipit.*" This sounds like sense. Few readers, at any rate, on going through the passage, would miss the presence of *non* if they did not find it there. If Aldus had not found it, I do not believe that he would have deliberately interpolated it. But if he found it in *P*, as I think he did, he retained it because it is right. Pliny does speak later of the places to which the freshet did not ascend (5); he there tells how much they suffered from the storm, even though out of the clutches of the freshet. But in the preceding part of the description, with which we are concerned, he mentions these places not to tell of the damage that befell them but to describe the view which their inhabitants had of the tumultuous freshet below them. They were too high (*excelsioribus*) to be in the path of the freshet, but they could observe its effects. And yet, they were not immune from harm, for the downpour of rain proved as bad for them as the ravages of the freshet on the lower ground. The reading *non deprehendit* is dramatic. We have the contrast between the raging flood and those who are watching it from above. With *non* omitted, the description loses force and, what is more, introduces a contradiction. We are told in section 5 that the freshet did not touch this higher ground, and yet we have just read that it rushed there too. The freshet of course came from above, but it tore its way to the lower level. It could not have covered the surrounding heights as well. Pliny, with the eye of an artist, selects these untouched heights and the view from them to give his description vividness, truth and depth. All this is lost if *non* disappears.

¹ Kortte cites *M* as having *qui* for *quos* after *uiderunt*. As neither Keil nor Mr. Merrill mentions this reading, I suppose that Kortte, despite his evident attention to *M*, must be wrong. If *M* does read *qui*, perhaps its original had *hi quos* rather than *quos*.

* 219, 4 (225, 15) *illa quidem loca*] See above, pp. 176 ff.

* 219, 22 (225, 33) *quoniam inexpectata*] *a inexpectata I expectata M*

The talk is of a wealthy person who, though apparently the prey of *captatores* during his lifetime, disappointed them all at his death by leaving his property to his relatives. "In a word, all the provisions of the will showed the utmost family feeling, and all the more as they were unexpected." (Melmoth.) This translates the text of Aldus, accepted by Keil: *in summa omnia pietate plenissima, ac tanto magis, quoniam inexpectata sunt*. The last part is a somewhat abbreviated expression for *tanto magis ea mireris quoniam inexpectata sunt*, as Gierig, unnecessarily, wished to read. It is also unnecessary to assume the omission of some longer phrase as Müller proposed. The reading of *M*, *tanto magis expectata* is obviously nonsense. Keil may well have thought that Aldus was (successfully) emending here, but now we find *inexpectata* confirmed by *I*; perhaps the *non defendit* reading which we have just examined is a parallel case. In the present reading, Aldus has been at least half-justified. Kortte emended *M* by adding "*duas tantum litteras*" and getting *tanto magis inexpectata*. This phrase has a meaning: "His legacies showed proper family feeling, and were therefore all the more unexpected." I prefer Aldus's *quoniam* (1) because he was not in the habit of tinkering with readings of *P* that made some sort of sense; (2) because *M* has a tendency to make omissions; (3) because the clause with *quoniam* seems to me neater than without.

219, 24-25 (226, 3-4) *ut qui de patre auo proauo quasi de orbo querantur*] (*quaerantur*) *M* (*ut om.*) *I* *qui de illo uti de patre auo proauo quasi orbi quaerantur* (*querantur a*²) *a*

The two texts are radically different. The talk is of the self-revelations made by the *captatores* in their criticisms of Domitius Tullus: "*seque ipsos, dum insectantur illum, turpissimis confessionibus produnt*." In the *MI* version, they betray themselves in complaining of one who was really father, grandfather and great-grandfather, as though he had died without a kinsman in the world. The implication is that Tullus was well supplied with relatives in three generations. In the text of Aldus, they betray themselves in complaining of him as though they were bereaved of a father or grandfather or great-grandfather — as the case might be. The implication is that the tears of the *captatores*

were copious and that their numerous array included three generations. Either reading has sense and point. Kortte (Longolius), who is disposed to follow *M* through thick and thin, adopts Aldus's *quasi orbi* (that is, the pith of his version), and for the first part reads either *ut qui de illo uti de* or *qui ut de*. I suspect that the *a*-text, plausible though it is, was reached by emendation. The point in the *MI*-text may not reveal itself to some readers immediately; it did not to keen-eyed critics like Catanaeus and Kortte. Aldus printed an emended text. Perhaps he made the conjecture himself; perhaps he accepted a conjecture of Giocondo's;¹ perhaps it was one of the additions by the second hand in *P*; perhaps it was an ancient conjecture incorporated in the text of *P* itself. The *PBF* family, while in general free from such imperfections, is not spotless. However, after stating these different possibilities, which I think deserve the stating, I will register this instance in the list of those readings of *P* which Aldus emended because he did not understand. Let us then adopt the reading of *M*. Mr. Merrill follows his Budaeus too reverently in omitting *ut*. This particle is supported by both *M* and *a*, and *ut qui* is neat. *I* has been guilty of omissions in other places.²

* 219, 26 (226, 5-6) quos sic decipere pro moribus temporum prudentia est] See above, pp. 176 ff.

220, 1 (226, 9-10) nam Curtilius Mancina perosus generum suum Domitium Lucanum (frater est Tulli) sub ea condicione filiam eius, neptem suam, instituerat heredem, si esset manu patris emissa. emiserat pater]

In this perfectly intelligible passage, Aldus interposes *ut* before *filiam* and changes *instituerat* to *institueret*. This makes a sentence without a verb if the last word in it is *emissa*. Aldus puts a comma after *emissa*, including *emiserat pater* in the sentence, securing grammar but abandoning sense. Curti(il)ius Mancina was certainly not the father of the young lady. This utter ruination of a good, clear text I should not call an attempt to emend. It rather suggests Aldus's tendency to

¹ Keil declared that the *quae si scabrae* clause 217, 27 (224, 8) was read by Aldus "*fortasse . . . non auctoritate codicis, sed coniectura Iucundi*." Mr. Merrill states with emphasis, but I know not by what authority, "*coniectura non Iucundi, ut Keil existimavit, sed Aldi ipsius*."

² See above, pp. 158 ff.

adhere even to the errors of his ancient source. Possibly the only error in *P* was *institueret* for *instituerat*, Aldus being responsible for *ut*, and missing the chance for a good conjecture.

220, 28 (227, 5) *digitos se seruorum*] *MI* *digitos seruorum a*. Aldus is wrong; *se* is neat; I think it was in *P*. But as the haplography is extraordinarily easy, either Aldus might have made it in preparing his copy or the printer in setting up the type. It is one of Aldus's offences according to Mr. Merrill's first list, but does not figure in his latest.

221, 1 (227, 9) *omnes fabulae Tullus*] *MI* *uenales tabulae a Cat.*²

There is no question that *MI* have the right reading here. Pliny says to his correspondent: *Habes omnes fabulas urbis; nam sunt omnes fabulae Tullus*. Tullus furnishes all the gossip that there is. "You have all the gossip, for Tullus's pictures are for sale" — this is pretty near to nonsense. It is strange that Catanaeus did not follow *M* in this passage. Kortte was the first to revolt against the Vulgate, with good sense. The only question is whether it would have occurred to Aldus to emend so satisfactory a text as that of *MI* had he found it in *P*. I think it more probable that he printed what he found, while either *I*^o, with discretion, did not write in this variant from *P*, or *I*, with discretion, did not repeat it.

I will add a case not in Mr. Merrill's list from a passage just beyond the portion of Book 8 recovered by Aldus from *P*, for here, too, the reading of *I* is accessible.

221, 5 (227, 13) *ne grauare*] *MI* *ne grauere r* *ne grauere scribere s* *ne grauare scribere a*

This is one of the instances which probably show that Aldus set up his text from one of the *s* editions collated with *P*. *Scribere* is clearly an emendation accepted by those editions. Aldus might either have thought that the word had been omitted in *P* or, not finding it there, had deleted it in a fashion not noticed by his compositor. The reading *grauare* for *grauere* Aldus could have found only by collating his basic edition with *P*.¹ On the same page is another reading which Aldus could have drawn from no other source than *P*: 221, 16 (227, 24) *intende libro*] *MIa* *intende librum meum r* *intende in librum meum s*.

¹ We need hardly suppose that Aldus suddenly turned to an inferior (and rare) manuscript like *c*, of the type of *r*, from which Mr. Merrill reports *grauare*.

These, and numerous other examples, show that Aldus was not merely consulting *P* in the more difficult passages, but was systematically collating it and following its authority even in places in which the current text was perfectly clear.

If *Ii* give us the nearest approach to *Parisinus*, from which Aldus deviates constantly and unnecessarily in favor of conjectures of his own, we should expect to find at least a few new and good readings in *Ii* contained neither in *a* nor in *M*, since that manuscript is of a different class. It is a considerable stretch of text — twelve of Kukula's pages — and yet I can not discover a single reading accepted by Mr. Merrill from *I* in which Aldus and *M* are divergently wrong, as they sometimes should be on this supposition. There are just two places in which they agree in what Mr. Merrill considers error, the right reading being furnished by *I* and printed in Mr. Merrill's text. One of these we have just examined (p. 185): 219, 24-25 (226, 3-4) *ut qui M qui I qui de illo uti a*. Here *I* drops an *ut* that ought not to be banished from the text. The other instance is

221, 1 (227, 9) *expectatur] expectatur Ma expectabatur I Merrill*

I hardly think that this innovation will find a permanent place in the text. The passage follows immediately after Pliny's statement that Domitius Tullus constituted the entire gossip of Rome. And now for the auction. "*Expectatur auctio*." This is the right text, not "We were waiting for the auction," before the old gentleman's death. It is hardly the proper context for the epistolary imperfect, which, if I mistake not, is not frequent in Pliny. And if he wished to use it here, why did he write "*Sunt omnes fabulae*" just before? *Expectabatur* is merely, I am afraid, one of the errors of *I*. It is not probable that the scribe introduced a conjecture. That is not the habit of *I* any more than it is of Aldus.

I believe, therefore, that we cannot point to a single good reading of *I* in the present stretch of text that is not either in *M* or in *a*. On the supposition that *a* is in general a faithful reproduction of *P* and that *I* is a faithful copy of a text in which *M* and *P* have been conflated, this condition of affairs is what we should expect. In discovering that there are no new readings in *I*, we do not thereby prove that it has nothing to contribute to the text. It is of very high value indeed, as the preceding pages abundantly show. Similarly, the Morgan

fragment contributes no new readings and yet, in my estimation, it has something more than a sentimental or a palæographical interest for the critic of Pliny's text.

9. *Aldus vindicatus*

So much for the evidence of Book 8. By Mr. Merrill's simple law, whereby concordance of *M* and *I* assures us of the reading of *P*, Aldus was clearly caught in at least a dozen cases of violent and unnecessary emendation. But now that law is proved invalid; the votes of *M* and *I*, if in agreement against *a*, are not as two to one; they count but for one, that of the *M*-text, or Class II, against one, that of Aldus, whose *Parisinus* is of Class I. I can now revert, therefore, to the hypothesis which I proposed and tested in a variety of ways in Part I of this paper. I am still engaged in testing it. Assuming that the Morgan fragment was a part of *P* and that Aldus elsewhere treated this codex with the respect which he has shown it in the portion included in II, I find no reason for abandoning this hypothesis for the one defended by Mr. Merrill, namely that as Aldus wilfully and flagrantly abandons satisfactory readings of *P* for conjectures of his own in the newly acquired part of Book 8, he cannot be trusted elsewhere to give a reliable report of *P*: *lectiones Parisini ex editione Aldina non restituentos*.

To revert to Mr. Merrill's longer list of indictments — and no longer list can be composed — I will again submit that the appended cases¹ are either misprints or scribal errors made by Aldus in preparing his copy for the press. In 215, 1 (221, 11) *quod*, Aldus preserves the error of *P*, as is attested by *I*. If *an abstinere*, 217, 9-10 (223, 20) is an interpolation, its insertion was prompted by the desire to retain, and to make intelligible, what is probably the wrong reading of *P* — the pith of the passage — which is also attested by *I*. A similar case is 216, 31 (222, 14) *prement*; Aldus starts with an obviously wrong reading of *P* which he and Budæus emend in different ways. In no less than eight cases, I believe that Aldus's text should be accepted. He

¹ 211, 24 (217, 28) *sedere*; 212, 7 (218, 12) *facundia*; 213, 6 (219, 13) *referenda*; 213, 9 (219, 17) *soleo*; 213, 26 (220, 3) *par*; 215, 2 (221, 12) *superfuit*; 220, 28 (227, 5) *se om*.

is faithfully reporting *P*, the readings of which are those of Class I and should here as often be preferred to those of Class II.¹

There remain three cases in which Aldus's reading is markedly different from that of *MI* and is pretty clearly wrong. But we can no longer say that he is wantonly deserting *P*, because the reading of this manuscript can no longer be revealed by Mr. Merrill's principle. The most startling case is 221, 1 (227, 9) *uenales tabulae Tulli*. Now in view of what we have seen of Aldus's conservative habits, of his scrupulous attention to the letter of his texts, of his determination to restore the Classics by saving them from the emender, I think that when we are confronted by so extraordinary a difference of readings as that in the present passage, we have a right to infer, unless contradictory data can be shown, that Aldus is not deserting a satisfactory reading of *P* for a stupid conjecture of his own, but is retaining the error that he found. In 220, 2 (226, 10) *ut . . . institueret*, where he makes a desperate attempt to save grammar at the expense of intelligibility, I do not think that he would have deviated from a perfectly clear passage in order to create the puzzle which he has so unsatisfactorily solved. Rather, the error, in whole or in part, was in *P*. In 219, 24 (226, 3) *qui de illo . . . uti . . . orbi*, we have deliberate emendation, if the reading of *MI* is correct as I think it is. But the excellence of that reading is not immediately evident; Catanaeus and Kortte preferred the Aldine. The text of *P* therefore might naturally have seemed to Aldus in palpable need of emendation, so that he printed a conjecture, either his own, or Giocondo's, or possibly one devised by some ancient scholar, whose reading got into *P*.

Instead, therefore, of being certain that Aldus has resorted to wilful and unnecessary conjecture at least a dozen times in a dozen pages of Teubner text, we should admit that it is exceedingly difficult to prove that he has resorted to conjecture at all. I think he may have done so in two or three cases of what I will again call the "obvious errors" of his ancient source, but I see no certain evidence, even in the *sortes*, as to what these cases are. In the instances of what I called misprints or scribal errors, it may be, though I think this improbable, that one or

¹ 214, 2 (220, 9) *ignorationem*; 217, 27 (224, 8) *quae si scabrae* etc.; 218, 31 (225, 10) *et fregit*; 218, 33 (225, 10) *uiderunt hi*; 219, 1 (225, 11) *non deprehendit*; 4 (15) *loca*; 22 (33) *quoniam inexpectata*; 26 (225, 5) *decipere*; 27 (6) *prudencia*.

two are intentional attempts at improvement. Of course if we consider, and it is our chief consideration, the accuracy with which the Aldine text reproduces *P*, Aldus has in these readings, whatever we call them, deviated from *P*. But he has not *intentionally* deviated. The error of these readings is obvious. I think we could easily recognize similar unsupported readings of Aldus elsewhere in the *Letters*. The unsupported readings that make good sense we should not immediately discard, on Mr. Merrill's principle, or if accepting them, accept them as Aldus's brilliant conjectures — brilliant for the nonce — but rather examine each in itself with reference to our general estimate of Aldus and the amount of freedom that he probably took in the making of conjectures. This estimate cannot be fully determined until we have examined the entire text of the *Letters*. My next article will be devoted to Book 10. If I omit this part of our problem in the present paper, it is because there are limits to the present volume; doubtless there are to the patience of my reader. My flight from the field at this point is not induced by fear or the writhings of a troubled conscience. The course of this argument finds itself again, perforce, at a moment of dramatic suspense. Meanwhile it will be apparent that what I call "a new approach" to the text of the *Letters* is an ancient and Aldine approach. To this we are conducted by what is indeed new evidence, furnished by the Morgan Fragment.¹

¹ I must add that it may well be necessary to alter my judgments of certain passages in the light of Carlsson's admirable discussion (see above, p. 80, N. 1). But the integrity of Aldus will, I think, appear all the clearer after this scrutiny. Before undertaking it, we should determine more exactly than has been done thus far, the relation of the eight-book family to the two other classes.

SUMMARIES OF DISSERTATIONS FOR THE DEGREE
OF PH.D., 1922-23

FREDERICK MASON CAREY. — *De Scriptura Floriacensi*

THIS dissertation is concerned chiefly with the history of the Abbey of Fleury (St. Benoît-sur-Loire), with the dispersion of its library when it was sacked by the Huguenots in 1562, and the development of its scriptorium. The monastery was founded near the middle of the seventh century, about one hundred years after the death of St. Benedict (c. 542), from whom it took its name — *coenobium sancti Benedicti Floriacensis*. Soon after the foundation, several monks of Fleury made a pilgrimage to the ruins of Monte Cassino and brought back the bones of their patron saint to rest in their abbey (c. 660). All our knowledge of the history of the place comes from the *Annales Floriacenses* and *Miracula Sancti Benedicti*, which were written by local monks. Theodulf, bishop of Orléans and abbot of Fleury (798-818), established a school in connection with the monastery, which enjoyed a great reputation through the Middle Ages, especially during the abbacy of Abbo at the end of the tenth century; it was under these influences that many manuscripts were produced and a great library established.

At the sacking of the abbey in 1562, Pierre Daniel, bailli of Fleury, saved many of the manuscripts by purchasing them from the soldiers; about two hundred and fifty codices were preserved in some unknown way and are to be found to-day in the public library of Orléans — only a few miles from their ancient home. Daniel's library has had a complicated history; it was bought by Jacques Bongars and Paul Pettau, from whose possession it was scattered all over Europe. Bongars's share came ultimately, through the hands of the Graviset family, to the Stadtbibliothek at Bern. The large part of Pettau's part came into the possession of Queen Christina of Sweden, and, with the rest of her library, finally to the Vatican, where her manuscripts formed the collection known as the *Codices Reginenses*. Isaac Vossius, her collector of ancient books, seems to have kept a large share of the Classical authors for himself; at any rate there are numerous Fleury

codices among the *Vossiani* in the public library in Leyden. There are about forty manuscripts also in the *Bibliothèque Nationale* in Paris, some of which were stolen by Libri from the library at Orléans and were later restored to the French government by Lord Ashburnham. Part of Pettau's library was bought by Lullin for the public library of Geneva; the remainder went to the collections of Mazarin, Joly, Bigot, Sequier, de Harlay, and Gaignières, all of which came eventually to the *Bibliothèque Nationale*. Stray volumes of the library of the Jesuits of Clermont found their way into the Meermann collection, and so to Oxford, Amsterdam, The Hague, and Berlin. Likewise the thefts of Libri have scattered other volumes in Florence and in England.

The attempt is here made not only to discuss the uncial and half-uncial hands, but also to classify the minuscule in eight different periods from the beginning of the ninth century through the eleventh. The Merovingian period is the first; it extends nearly to the end of the eighth century. The rough, cursive traits of the style known as Merovingian are beginning to give way to the Caroline minuscule. In the next period, which goes to about the year 815, the Caroline minuscule has almost displaced the earlier hand. Between the years 815 and 825, the influence of the script of Tours is beginning to be felt; side by side are to be found the new Tours letters and abbreviations, and the older traits of Fleury. One characteristic of the Tours style which Fleury never accepted was the elimination of ligatures; instead, they were embellished. Between 825 and 845 the full influence of the Tours school can be followed; the chief trait of this period is the half-uncial *a* of Tours, large, rounded, and closed. From the middle of the century to about 880, the full Tours hand is gradually displaced by a more crabbed script in which the letter *a* is again an important test; it is now open and now flat-topped. From 880 to the end of the century, the crabbed traits gradually disappear; it is by means of the ligatures that this period is determined.

The first half of the tenth century offers a full, ample, Caroline hand, with slight traces from the preceding periods, and, on the whole, with few special characteristics. About the middle of the century there is a reversion to the writing of the ninth century; at first this archaistic revival manifests itself gently, but soon breaks out into very fantastic forms, which are easily differentiated from the imitated traits of the

preceding century. The eleventh century offers a rough, ungraceful script; the main criterion of this period is the letter *g*. On the whole, the script of Fleury is of a very small and delicate variety; the influence of Tours was not permanent. It was soon superseded by a style which, though inferior, was worked out in a satisfactory and even beautiful fashion.

EVA MATTHEWS SANFORD.—*Quibus Rationibus Auctorum Latinorum Opera in Libris Manuscriptis Collecta Sint*

THE purpose of this thesis is to explain the nature, value, and use of the collections of extracts, "*florilegia*," and of entire works, found in a large number of mediaeval manuscripts. As such books were compiled chiefly in the Carolingian age, it seems appropriate to refer to them as *libri manuales*, a term used by Alcuin. These manuscripts are often cited in editions of various works, but very rarely is anything said of their contents aside from the particular work with which the editor is concerned. It is my belief, derived from a study of the contents of several hundred of these books, that a consideration of them as a whole is of much importance for the history of the transmission of classical authors and of the parts which they played in mediaeval education. The *libri manuales* form a long series reaching through the Renaissance. I have limited discussion mainly to those compiled from Charlemagne's time, or in a few cases earlier, through the thirteenth century. The quantity of important manuscripts of this century containing selections from the classical authors may serve as evidence of the humanistic interests of a period in which, according to certain scholars, humanism went into a decline.

I have attempted to account for the compilation of these books by relating them to the educational ideals of the periods to which the manuscripts belong, and to compare them with the earlier types of such collections. Especial attention is devoted to Alcuin and to certain collections whose compilers are known, namely those of Sedulius Scottus, Heiric, Hadoard, and Richard de Fournival. The continuity of tradition represented by these collections proves that these were by no means unique in their several times, but that other unknown men cherished the same spirit in their study of the Classics. In all,

over three hundred manuscripts are classified, as well as many books now lost which are listed in the mediaeval catalogues. They are distributed among the following groups:

I. Manuscripts containing works on grammar, generally combined with the writings of one or more classical authors.

II. Manuscripts which contain works on the other liberal arts, many of which are themselves by ancient authors. History is seen to be a separate branch of study on a plane with the other arts. The list of the arts is that of Varro in many cases, instead of the more limited seven arts which we ordinarily associate with the Middle Ages, since medicine and architecture are often included.

III. The remaining groups deal almost entirely with literature in itself. Of the books which include both ancient and mediaeval or earlier Christian works some seem to be intended for use as handbooks of poetry; a fair number contain only one ancient and one Christian author's works, and there are two excellent examples of manuscripts containing a collection of songs with notes. I have grouped separately the *libri Catoniani* which were intended primarily for school use; other books seem clearly designed for more advanced study. Christian anthologies similar to the others are added, and a fair number of books indicate that the philosophical works of Seneca and Cicero, Cato's moral distichs and Avianus's beast fables were scarcely distinguished from Christian works by their compilers.

IV. There follow manuscripts which contain the works of two or more ancient authors with no mediaeval additions; books containing one considerable work with numerous lesser additions, such as the manuscripts of Virgil that contain an anthology of opuscula about him or attributed to him; the collections of *accessus* or glosses, commentaries and lives of the authors; collections of *exempla* chosen from different authors for the purpose of establishing the metrical quantities of certain words, and the earlier of the collections of *proverbia* or *sententiae* from ancient and mediaeval authors which continued to be made in great numbers through the sixteenth century.

It will be noted that books which do not include any classical works are frequently described with the others, for the same spirit ran through them to such an extent that it is impossible to draw the line closely. This very fact is of the greatest significance and shows that

the distinction was not always carefully drawn between the strictly classical authors and the earlier Christians, or in fact any authors of interesting and valuable Latin works. The humanists who compiled these books were not literary Puritans. Further, practically all of these books would fill practical needs in the schools and monasteries, and yet they are also books for the general reader and sometimes seem directly intended for the man of good literary taste but little leisure, or perhaps with too slender a purse or too intense a thirst for general knowledge to be satisfied with the ponderous complete editions of separate authors — a provision that is not without a parallel in our own day. Some of the books also, like the notebooks of Sedulius in the ninth century, and Boccaccio in the fourteenth, while to the casual reader they may seem mere miscellanies, are really the reverse, as they reflect in small compass the varied and harmonious interests of a student and lover of literature in one age or another. These books, then, which some say were copied out with no thought of their content merely for the sake of exercise in writing, may prove to give valuable indication of the taste of their writers. In general, they furnish ample refutation of the more narrow and hasty dicta concerning the mediaeval attitude towards classical culture.

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